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THE

PHILOSOPHICAL MAGAZINE AND JOURNAL:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND COMMERCE.

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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CONTENTS

OF THE SIXTY-EIGHTH VOLUME.

CONTENTS.

CONTENTS.

PLATES.

PLATES.

ERRATUM

In Mr. Bevan's Table relative to the Cohesion of Wood, p. 270, No. 34, *for* Linc. log, *read* Linc. bog.

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *JULY* 1826.

I. *On the Ellipticity of the Earth as deduced from Experiments made with the Pendulum. By* J. Ivory, *Esq. M.A. F.R.S.**

THE experiments made with the pendulum are now very numerous, and the great difficulty is to reconcile them, and to deduce from them the best-founded result with respect to the figure of the earth. Hitherto it has been thought sufficient to proceed on the theory delivered by Clairaut, neglecting the square and higher powers of the ellipticity. The reason is, that the square of the earth's ellipticity is very small, and at least of the same order with the unavoidable errors of the experimental quantities, and in all probability even of a magnitude much less considerable than those errors. As this remark appears to be well founded, it can hardly be expected that any practical advantage will be gained by solving the problem to a greater degree of approximation than Clairaut has done. But in order to satisfy the present restlessness of inquiry, and to confine, if possible, the attempts of philosophers on this subject within proper bounds, the point is perhaps not undeserving of examination.

The equilibrium of a fluid, or of different fluids varying in density, covering a nucleus is a problem of easier solution than the equilibrium of a homogeneous fluid. In the former case we have in the interior of the fluid a given surface which determines the figure of all the strata above it. In the latter case there is nothing in the interior which can assist us in finding the figure necessary to the equilibrium of the mass.

The equilibrium of a mass entirely fluid, but consisting of strata of different densities, comes under the general case of a nucleus. For, however the density varies from stratum to

* Communicated by the Author.

to stratum, there must be a small central body which may be considered as of uniform density, and which, like a nucleus, will mould the figure of the rest of the mass. This case therefore is in no respect different from a nucleus, except that it is requisite to know previously the figures of equilibrium of a homogeneous fluid.

There is an essential distinction between the equilibrium of a fluid of variable density, covering a nucleus, and that of a homogeneous fluid, which takes place when the particles of the fluid attract one another, as happens in the planets. In the first case the conditions of equilibrium bind together the whole fluid mass between the outer surface and the nucleus. If an additional stratum be spread over the external surface, the conditions of equilibrium cannot be adjusted unless the whole fluid mass to which the stratum is added, undergo a modification of its figure. And if an external stratum of equal pressure be taken away, the remaining fluid must change the figure of its surface in order to restore the equilibrium. The thickness of the fluid above the nucleus, or the dimensions of the whole body, is a necessary element of the equilibrium.

But it is otherwise with respect to the equilibrium of a homogeneous fluid. Supposing a body of this description having any given dimensions to be *in equilibrio*, we must conceive that there do exist bodies of less and less dimensions that would be *in equilibrio* if subjected to the action of similar forces. Hence we must infer, that if a homogeneous fluid *in equilibrio* be divided by level surfaces into strata of equal pressure, every internal body bounded by a level surface will be *in equilibrio* separately and independently of the exterior fluid. If a level stratum be taken away, the remaining fluid will be merely lightened of a pressure, but its equilibrium will not be disturbed. On the other hand, if a like stratum be added, the new body will be *in equilibrio* without any change of the surface upon which the stratum is laid. Now the pressure which any thin exterior stratum exerts upon the fluid below it, is the joint effect of two causes; of the gravitation at the separating surfaces which acts upon the matter of the stratum, and of the attraction of the stratum upon the interior fluid; and it is manifest that the interior fluid body cannot be *in equilibrio* independently of the stratum, unless there be a separate equilibrium for each of the two forces. But if the action of the exterior stratum does not disturb the equilibrium of the interior fluid body, this can happen only because the resultant of the attractions of the exterior matter upon any particle within the stratum, is evanescent. This law, therefore, must actually

actually take place in a homogeneous fluid *in equilibrio*, and it is sufficient to determine the figure which it will assume.

In the particular case of a homogeneous fluid *in equilibrio* by a centrifugal force, and the attraction of its particles in the inverse proportion of the square of the distance, the same conclusion may be obtained in a different way. It is easy to prove, from the nature of the forces in action, that in such a body all the level surfaces are similar to one another; and this property, while it leads immediately to the law already laid down, is of itself sufficient to determine the figure which is necessary to the equilibrium of the fluid.

The theory we have been explaining has been opposed, and has been rejected superciliously without examination. But it is founded in truth, and will ultimately be adopted. No other way but by investigating the physical properties of equilibrium, can be successful in simplifying a very difficult subject, and in rendering it completely satisfactory.

But the fact is, that, supposing an attraction according to the law of nature and a centrifugal force, there is no other figure known which is competent to the equilibrium of a homogeneous fluid, except the elliptical spheroid of revolution. Such therefore must be the figure of the central body, or nucleus, of a mass entirely fluid, but consisting of strata of variable density. When the body is nearly spherical Clairaut has already shown that the equilibrium requires it to be an oblate spheroid; for it is manifest the method of solution followed by that geometer coincides with the view of the problem taken here. Clairaut has carried his calculations no further than the first power of the ellipticity; and in pushing the approximation so as to include the square of the same quantity, I have obtained the following equations, taking care to exhibit them in as simple a form as possible. The symbols ϱ, e, denote the density, and the double of the ellipticity, both being functions of a, the axis of revolution of a stratum of equal density, varying from the centre to the outer surface of the fluid: A is an unknown function of the same variable quantity.

$$
\begin{aligned}
0 = {} & \frac{e}{3} \cdot \frac{\int \varrho\, d \,.\, a^3}{a^3} - \frac{1}{5} \cdot \frac{\int \varrho\, d\,(a^5 e)}{a^5} - \frac{q}{3} - \frac{1}{5} \left\{ \mathrm{F} - \int \varrho\, d e \right\} \\
& - \frac{2}{7} e \left\{ \mathrm{F} - \int \varrho\, d e \right\} + \frac{3}{35} \left\{ \mathrm{G} - \int \varrho\, d \,.\, e^2 \right\} + \frac{e}{3} \cdot \frac{\int \varrho\, d\,(a^3 e)}{a^3} \\
& + \frac{e}{7} \cdot \frac{\int \varrho\, d(a^5 e)}{a^5} - \frac{5 e^2}{21} \cdot \frac{\int \varrho\, d \,.\, a^3}{a^3} - \frac{1}{5} \cdot \frac{\int \varrho\, d\,(a^5 e^2)}{a^5} - \frac{8}{21} q e. \\
& \frac{\mathrm{A}\, e^2}{2} \cdot \frac{\int \varrho\, d\, a^3}{a^3}
\end{aligned}
$$

$$\frac{A e^2}{2} \cdot \frac{\int \varrho d \cdot a^3}{a^3} - \frac{1}{6} \cdot \frac{\int \varrho d (a^7 A e^2)}{a^7} = \frac{12 e}{175} \cdot \left\{ F - \int \varrho d e \right\}$$

$$+ \frac{a^3}{3} \cdot \left\{ H - \int \varrho d \left(\frac{A e^2}{a^2} \right) \right\} - \frac{e^2}{35} \cdot \frac{\int \varrho d \cdot a^3}{a^3}$$

$$- \frac{18 e}{175} \cdot \frac{\int \varrho d (a^5 e)}{a^5} + \frac{3}{35} \cdot \frac{\int \varrho d (a^5 e^2)}{a^5} + \frac{4}{35} q e.$$

All the integrals begin at the centre of the fluid mass and terminate at the surface; F, G, H, denote the whole integrals accumulated between the limits mentioned; and $q = \frac{1}{\frac{4}{3}\pi} \times \frac{4\pi^2}{T^2}$, T representing the time of a revolution, and $\pi = 3{\cdot}1416$.

If we reject all the terms containing e^2, there will remain only the first line of the first equation, which is no other than the solution obtained by Clairaut. If we make ϱ constant, we shall find $A = 0$; and the first equation will give the relation between e and q, pushed to quantities of the second order on the supposition that the fluid is homogeneous. The quantity A, therefore, begins at the surface of the nucleus and extends to the outer surface of the fluid. From what has been said there is no doubt that the two equations are consistent and possible.

But if the equations be possible, they are not easily solved; and besides, we are ignorant of the law of density in the interior of the earth. All therefore that can be done, is to form the expressions of the radius of the earth at any latitude, and of the force of gravity which prevails there, in order to compare them with the measurements of the meridian and the variation of gravity as determined by means of the pendulum. Now the polar semiaxis being unit, and λ the latitude, if we put,

$$\varepsilon = \frac{e}{2} - \frac{e^2}{8} - \frac{5}{16} A e^2,$$

the radius of the earth, or r, will be thus expressed,

$$r = 1 + \varepsilon \cos \lambda + \left(\frac{5}{8} \varepsilon^2 - \frac{35}{16} A \varepsilon^2 \right) \sin^2 2\lambda :$$

and if g and G be the gravity at the latitude λ and at the equator, then φ denoting the proportion of the centrifugal force to gravity at the equator, and $\frac{5}{2} \varphi = {\cdot}00865$; we shall have,

$$g = G \left\{ 1 + \left(\frac{5}{2} \varphi - \varepsilon + \frac{\varepsilon^2}{2} - \frac{17}{14} \varphi \varepsilon + \frac{5}{2} A \varepsilon^2 \right) \sin^2 \lambda \right.$$

$$\left. + \left(\frac{\varepsilon^2}{8} - \frac{5}{8} \varphi \varepsilon - \frac{105}{16} A \varepsilon^2 \right) \sin^2 2\lambda \right\}.$$

It

It is only with the latter of these equations that we are to be occupied at present, and for the sake of simplicity we shall write it a little differently. Let,

$$M = \varepsilon - \frac{\varepsilon^2}{2} + \frac{17}{14}\varphi\varepsilon - \frac{5}{2}A\varepsilon^2$$

$$N = \frac{\varepsilon^2}{8} - \frac{5}{8}\varphi\varepsilon - \frac{105}{16}A\varepsilon^2;$$

then, $1 + \frac{5}{2}\varphi\sin^2\lambda - \frac{g}{G} = M\sin^2\lambda - N\sin^2 2\lambda.$

I now take the following results of Captain Sabine's experiments from the Quarterly Journal of Science, No. 39, p. 103, viz.

Stations.	Latitude.	Pendulum inches.
Maranham . .	2° 31′ 43″	$39{\cdot}01214 = l^{(1)}$
London . . .	51 31 8	$39{\cdot}13910 = l^{(2)}$
Drontheim . .	63 25 54	$39{\cdot}17456 = l^{(3)}$
Hammerfest . .	70 40 5	$39{\cdot}19519 = l^{(4)}$
Greenland . .	74 32 19	$39{\cdot}20335 = l^{(5)}$
Spitzbergen . .	79 49 58	$39{\cdot}21468 = l^{(6)}$

The first of these places is very near the equator, and the others are so far removed from it that the variation of the pendulum may be supposed very great in proportion to the errors of observation. Putting L for the equatorial pendulum, and observing that gravity is proportional to the length of the pendulum, the foregoing formula will give us the following equations in numbers, viz.

$$1{\cdot}0000168 - \frac{l^{(1)}}{L} = {\cdot}001946\,M - {\cdot}007771\,N$$

$$1{\cdot}0053007 - \frac{l^{(2)}}{L} = {\cdot}612797\,M - {\cdot}949107\,N$$

$$1{\cdot}0069196 - \frac{l^{(3)}}{L} = {\cdot}799954\,M - {\cdot}640110\,N$$

$$1{\cdot}0077021 - \frac{l^{(4)}}{L} = {\cdot}890412\,M - {\cdot}390313\,N$$

$$1{\cdot}0080351 - \frac{l^{(5)}}{L} = {\cdot}928930\,M - {\cdot}264075\,N$$

$$1{\cdot}0083805 - \frac{l^{(6)}}{L} = {\cdot}968840\,M - {\cdot}120756\,N$$

We must next exterminate from these equations, the term containing the unknown quantity L. For this purpose, multiply the first equation by $\frac{l^{(2)}}{l^{(1)}} = 1{\cdot}0032543$; then,

$$1{\cdot}0032711 - \frac{l^{(2)}}{L} = {\cdot}001953\,M - {\cdot}007796\,N;$$

and

and this equation being subtracted from the second, we get,

$$\cdot 0020296 = \cdot 610844\, M - \cdot 941311\, N.$$

And in like manner by combining the first equation with each of the remaining four, the following results will be obtained, viz.

$$\begin{aligned} \cdot 0027395 &= \cdot 798000\, M - \cdot 632307\, N \\ \cdot 0029932 &= \cdot 888457\, M - \cdot 382506\, N \\ \cdot 0031170 &= \cdot 926974\, M - \cdot 256266\, N \\ \cdot 0031716 &= \cdot 966884\, M - \cdot 112945\, N. \end{aligned}$$

These are the equations from which we are to find M and N; but it may be proper to inquire previously, what degree of exactness we ought to assign to the results. All the numbers in the equations may be considered as exact, except the quotients of the pendulums, the exactness of which will depend upon the accuracy of the experiments. Now it may be affirmed that, when the pendulum is expressed in English inches, there is no one instance in which we are quite sure of the figure in the fourth decimal place, and all the following figures are uncertain.

In the Phil. Trans. for 1818, the length of the pendulum in London is found equal to 39·13860; but there is a necessary correction of ·00079 to be added, on account of an omission in estimating the specific gravity of the pendulum, which makes the length 39·13939 (Phil. Trans. for 1819, p. 415). It is then said, *the allowance of* ·00031 *for the height above the level of the sea, should, according to Dr. Young, be only* ·00021; and this brings the pendulum to 39·13929. There is thus a unit of uncertainty in the fourth place of figures, arising from a difference of opinion about a necessary reduction; and we should be warranted in concluding that the length of the pendulum cannot possibly, from the nature of the case, be ascertained beyond the degree of exactness we have assigned, whatever be the skill and care of the experimenter. In the Journal of Science, No. 39, p. 102, the length of the pendulum is stated at a mean equal to 39·13910, which is the number we have adopted.

The length of the decimal pendulum at Paris, determined by Borda, when all reductions are made, is, in parts of a metre, 0·741904*; and the same length, according to the later experiments of MM. Biot, Mathieu, and Bouvard, is 0·74191749†. The difference is ·00001349 of a metre, or ·00053 of an inch. The later determination is no doubt the more exact; yet it cannot be said that this instance forms an exception to what we have affirmed.

* *Conn. des Tems.* 1816, p. 321.

† Supp. Encycl. Brit. vol. vi. p. 134.

We

We shall only add the pendulum at Unst, which has been determined both by Captain Kater and M. Biot, and which the latter thinks is one of the most certain that has yet been found experimentally. The two results, however, are different ·000007 of a metre, or ·00027 of an inch*.

Now if the lengths of the pendulums be uncertain in the fourth and following places of figures, the same must be true of the quotients of the lengths, and of all the small numbers on the left-hand side of the equations for finding M and N. It appears, therefore, that the values found by those equations cannot be reckoned quite sure in the fourth place of figures, and are uncertain in all the following figures.

Divide all the terms of each of the equations mentioned by the coefficient of M; then

$$\begin{aligned} M &= \cdot 003322 + 1\cdot 541\ N \\ M &= \cdot 003432 + \cdot 792\ N \\ M &= \cdot 003369 + \cdot 430\ N \\ M &= \cdot 003362 + \cdot 276\ N \\ M &= \cdot 003280 + \cdot 117\ N. \end{aligned}$$

If we neglect quantities of the second order as in the usual solution of the problem, then $N = 0$, and M will coincide with the ellipticity, of which, therefore, we have five different values derived from independent experiments. These values agree very well with the remarks that have been made, and they likewise prove the accuracy of the experiments. For great care must have been taken to insure accuracy in such delicate operations, when the extremes of five independent results differ from one another less than $\frac{1}{20}$ of the whole, and from the mean quantity only $\frac{1}{46}$ of the whole. It is plainly impossible to improve the solution by assigning any value whatever to N. The experimental quantities leave the numbers uncertain in the fourth and fifth places of figures, and the same numbers should be exact, at least to the sixth place of figures inclusively, in order to determine with any consistency quantities of the second order. We may therefore safely conclude that Clairaut's theory, without being carried to any greater approximation, is fully sufficient in all our researches concerning the figure of the earth.

In order to confirm the foregoing reasoning, I shall conclude with adding two other instances, both of great accuracy, viz.

	Latitude.	Pendulum.
Paris . . .	48° 50′ 14″	39·12930
Unst . . .	60 45 28	39·17146

* Supp. Encyc. Brit. vol. vi. p. 130.

By proceeding with these data in the same manner as before, I have found,

$$M = 0{\cdot}003333 + 1{\cdot}720\ N$$
$$M = 0{\cdot}003272 + {\cdot}947\ N.$$

Neglecting the terms containing N, we have two new determinations of the earth's ellipticity, which agree very well with the former quantities. The mean of all the seven results is extremely near $\frac{1}{300}$, coinciding almost exactly with what we obtain from the pendulums in London and Paris. It is certainly remarkable that all the seven results fall between ·00326, which is the ellipticity adopted in France, and ·00346, which is the like quantity deduced by Captain Sabine from his own experiments.

July 4, 1826. J. Ivory.

II. *On the Equilibrium of a Fluid attracted to a fixt Centre.* *By* J. Ivory, *Esq. M.A. F.R.S.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

Taking a hint from the Quarterly Journal of Science just published, the extreme case of a fluid *in equilibrio* by a centrifugal force and the attraction of its particles to a fixt centre, will illustrate the investigations I sent you in the beginning of the month. This is the view of the figure of the earth adopted by Huygens, and is equivalent to an infinite density at the centre; whereas Newton supposed an attractive force diffused uniformly from particle to particle through the whole mass. As Clairaut has solved the problem, we may take the solution of it in his book *, viz.

$$\frac{\pi}{n+1}(x^2+y^2)^{\frac{n+1}{2}} - \frac{1}{2}\cdot\frac{f}{r}\cdot y^2 = A,$$

in which equation, π is the intensity of the central force; $\frac{f}{r}$ the measure of the centrifugal force; n the index of attraction; and x, y, the coordinates of a point in the surface of the fluid, x being parallel, and y perpendicular, to the axis of revolution. Now let $\pi = 1$; $n = -2$; $\frac{f}{r} = \varphi = \frac{1}{289}$; $x = R \sin\theta$, $y = R\cos\theta$; then,

$$\frac{1}{R} + \frac{\varphi}{2}R^2\cos^2\theta = A.$$

If the figure of the fluid be nearly spherical, we may sup-

* *Théorie de la Figure de la Terre*, part. i. § xxix.

pose

pose $R = 1 + u$, u being a small quantity; then $A = 1$, and we readily get,

$$\frac{\varphi}{2} \cos^2 \theta = \frac{u}{(1+u)^3}.$$

For a first approximation, $u = \frac{\varphi}{2} \cos^2 \theta$, $R = 1 + \frac{\varphi}{2} \cos^2 \theta$; and the fluid is an elliptical spheroid of revolution, the ellipticity being $\frac{\varphi}{2}$, whereas, in the theory of Newton, it is $\frac{5}{4} \varphi$.

Carrying the approximation a step further, we have,

$$\frac{\varphi}{2} \cos^2 \theta = u - 3u^2;$$

and hence $\quad R = 1 + u = 1 + \frac{\varphi}{2} \cos^2 \theta + \frac{3\varphi^2}{4} \cos^4 \theta;$

or, which is the same thing,

$$R = 1 + \left(\frac{\varphi}{2} + \frac{3\varphi^2}{4}\right) \cos^2 \theta - \frac{3\varphi^2}{16} \sin^2 2\theta.$$

The figure of the fluid is no longer elliptical. Its surface falls every where below an elliptical spheroid having the same polar and equatorial diameters. The excess of the radius of the equator above the semi-polar axis, is increased from $\frac{\varphi}{2}$ to $\frac{\varphi}{2} + \frac{3\varphi^2}{4}$, or it has received an addition of about $\frac{1}{200}$ of its former quantity.

In general when the problem of the figure of the earth is pushed so as to include quantities of the second order, the ellipticity will receive a variation which may amount to $\frac{1}{200}$ of the first approximation. But as the skill of the experimentalist is insufficient, and in all probability will ever remain insufficient, to determine the earth's ellipticity nearer than $\frac{1}{40}$th or $\frac{1}{30}$th part, it can be of no avail to be solicitous about a greater degree of theoretical accuracy. The theory of Clairaut is proportionate to the practical knowledge that can be acquired; to go further belongs to that species of philosophy which strains at gnats and swallows camels.

I remain, sir, &c. &c.

July 11, 1826. James Ivory.

III. *Researches on the Theory of Hydro-dynamics. By* Thomas Tredgold, *Esq. Civil Engineer**.

In this paper I propose to make a few remarks on the propositions which are commonly given as the basis of the theory of the resistance of fluids. It is well known that the

* Communicated by the Author.

results derived from these propositions differ from the results of experiment; and it is so important that the resistances of fluids should be established on true principles, that one naturally expects, the subject to have been investigated with more than ordinary care. Under such an impression it is not without anxiety, as to the result, that I am about to call in question the reasoning of these propositions.

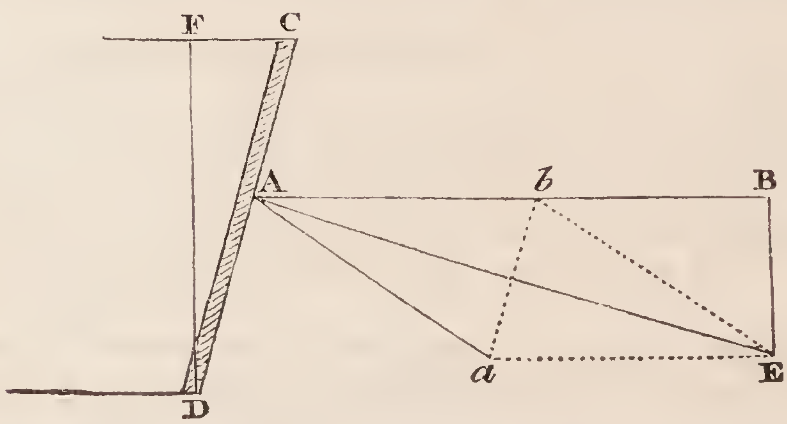
All the propositions alluded to, may be referred to Prop. 34, book ii. of Sir Isaac Newton's Principles of Natural Philosophy. Hence, if I succeed in showing that the demonstration of that proposition is not correct, the rest must of course, in as far as they depend on the same reasoning, have the same source of error.

The proposition is as follows:—" If in a rare medium, consisting of equal particles freely disposed at equal distances from each other, a globe and cylinder, described on equal diameters, move with equal velocities in the direction of the axis of the cylinder, the resistance of the globe will be only half as great as that of the cylinder."

The demonstration of this proposition in the Principles of Natural Philosophy depends on its being proved that the efficacy of a particle to resist the motion of a surface is as the square of the sine of the angle of inclination of that surface to the direction of its motion. Now I contend that this is not true, but that the real ratio of the resistance is simply as the *sine* of the angle of inclination.

For let CD be the plane moving with an equable velocity, in the direction AB, in a fluid at rest; DF being the breadth of the moving plane in a direction perpendicular to the direction of its motion, and CAB the angle the plane makes with its direction.

Let AB be the direct resistance necessary to render the motion of the plane equable at the given velocity; and draw AE perpendicular to the plane, and BE perpendicular to AB;



then AE is the pressure perpendicular to the surface of the plane, and the actual pressure of the fluid on the plane must be to

to the effective resistance as AE is to AB; consequently, the actual pressure of a fluid on a plane being the same, its resistance will vary as AB, or as the sine of the angle CAB.

The omission in the demonstration of Prop. 34 of the Principles consists in taking the resistance of a particle in a direction opposite to the motion of the body as its whole effect. Whereas, in consequence of the continuity of the fluid, reflection cannot take place, and the actual resistance in a direction perpendicular to the surface must be composed of two parts, viz.—the force in the direction of the motion, and the force by which reflection is prevented. This leads us to a valuable theorem in inquiries of this kind.

Prop.—If a plane surface moves with an equable velocity in a fluid, the height of a column of that fluid which would generate that velocity is to the height of a column equivalent to the resistance of the fluid as the square of the radius is to twice the square of the sine of the angle the plane makes with the direction of its motion.

The resistance is as AB, but the force of the fluid is as EA; and, if this force be decomposed, we have its two parts Ab, and Aa, of which Ab is proportional to the height of a column of the fluid which is due to the velocity of the plane's motion; and Aa the resistance to reflection.

But the angle of reflection being equal to the angle of incidence, and the triangles similar,

$$\text{Rad.} : \sin \text{CAB} :: \text{A}b : \tfrac{1}{2}\text{AE};$$

whence $$\text{AE} = \frac{2\,\text{A}b \times \sin \text{CAB}}{\text{rad.}}.$$

Again, rad. : sin CAB : : AE : AB, or $\text{AB} = \frac{2\text{A}b \times \sin^2 \text{CAB}}{\text{rad.}^2}$;
and, consequently, $\text{A}b : \text{AB} :: \text{rad}^2 : 2 \sin^2 \text{CAB}$.

When the plane is moved in a direction perpendicular to its surface, Ab becomes half AB, or the resistance is double the weight of the column due to the velocity. This distinction between the resistance and the head due to the velocity is most important; and the fact of its coinciding with the result previously obtained for the particular case when the motion is perpendicular to the plane, will have its weight in procuring the theory of hydraulics here laid down some degree of attention.

The comparison of the resistance of a cylinder and a globe requires that a further research should be developed, as the plane's motion and the pressure of the fluid which follows must be taken into consideration. Were we to neglect these, or simply wished to compare the direct resistance of a globe and cylinder of equal diameters, the ratio would be as 2 is to 3, instead of as 1 to 2.

IV. *An*

IV. *An Account of some Geological Specimens, collected by Captain* P. P. King, *in his Survey of the Coasts of Australia, and by* Robert Brown, *Esq., on the Shores of the Gulf of Carpentaria, during the Voyage of Captain* Flinders. *By* William Henry Fitton, *M.D. F.R.S. V.P.G.S.**

THE following enumeration of specimens from the coasts of Australia, commences, with the survey of Captain King, on the eastern shore, about the latitude of twenty-two degrees, proceeding northward and westward: and as the shores of the Gulf of Carpentaria, previously surveyed by Captain Flinders, were passed over by Captain King, Mr. Brown, who accompanied the former, has been so good as to allow the specimens collected by himself in that part of New Holland, to supply the chasm which would otherwise have existed in the series. Part of the west and north-western coast, examined by Captain King, having been previously visited by the French voyagers, under Captain Baudin, I was desirous of obtaining such information as could be derived from the specimens collected during that expedition, and now remaining at Paris; although I was aware that the premature death of the principal mineralogist, and other unfavourable circumstances, had probably diminished their value†:—But the collection from New Holland, at the School of Mines, with a list of which I have been favoured through the kindness of Mr. Brochant de Villiers, relates principally to Van Diemen's Land; and that of the Jardin du Roi, which Mr. Constant Prevost has obliged me with an account of, does not afford the information I had hoped for. I have availed myself of the notices relating to physical Geography and Geology, which are dispersed through the published accounts of Captain Flinders' ‡, and Baudin's Voyages§; and these, with the collections above alluded to, form,

* From Captain King's Narrative of a Survey of the Intertropical and Western Coasts of Australia, 1826, vol. ii. p. 566.

† M. Depuch, the mineralogist, died during the progress of the voyage, in 1803; and, unfortunately, none of his manuscripts were preserved. M. Péron, the zoologist, after publishing, in 1807, the first volume of the account of the expedition, died in 1810, before the appearance of the second volume. Voyage, &c. i. p. 417, 418; and ii. p. 163.

‡ A Voyage to Terra Australis, &c., in the years 1801, 1802, and 1803, by Matthew Flinders, Commander of the Investigator. Two vols. quarto, with an atlas, folio; London, 1814.

§ *Voyage de Découverte aux Terres Australes, &c.*—Tome i. redigé par M. F. Péron, naturaliste de l'Expedition;—Paris, 1807.—Tome ii. redigé par M. Péron, et M. L. Frecinet, 1816.—A third volume of this work, under the title of *Navigation et Géographie,* was published by Captain Freycinet, in

form, I believe, the only sources of information at present existing in Europe, respecting the geological structure and productions of the north and western coasts of Australia.

In order to avoid the interruption which would be occasioned by detail, I shall prefix to the list of specimens in Captain King's and Mr. Brown's collections, a general sketch of the coast from whence they come, deduced, principally, from the large charts*, and from the narratives of Captains Flinders and King,—with a summary of the geological information derived from the specimens. But I have thought it necessary to subjoin a more detailed list of the specimens themselves; on account of the great distance from each other of many of the places where they were found, and of the general interest attached to the productions of a country so very remote, of which the greater part is not likely to be often visited by geologists. The situation of such of the places mentioned, as are not to be found in the reduced chart annexed to the present publication, will be sufficiently indicated by the names of the adjacent places.

General Sketch of the Coast.

The north-eastern coast of New South Wales, from the latitude of about 28°, has a direction from south-east to north-west; and ranges of mountains are visible from the sea, with little interruption, as far north as Cape Weymouth, between the latitute of 12° and 13°. From within Cape Palmerston, west of the Northumberland Islands, near the point where Captain King began his surveys, a high and rocky range, of very irregular outline, and apparently composed of primitive rocks, is continued for more than one hundred and fifty miles, without any break; and after a remarkable opening, about the latitude of 21°, is again resumed. Several of the summits, visible from the sea, in the front of this range, are of considerable elevation:—Mount Dryander, on the promontory which terminates in Cape Gloucester, being more than four thousand five hundred feet high. Mount Eliot, with a peaked summit, a little to the south of Cape Cleveland, is visible at twenty-five leagues' distance; and Mount Hinchinbrook, immediately upon the shore, south of Rockingham Bay, is more than two thousand feet high. From the south of Cape Grafton to Cape Tribulation,

in 1815. It contains a brief and clear account of the proceedings of the expedition; and affords some particulars connected with the physical geography of the places described, which are not to be found in the other volumes.

* These charts have been published by the Admiralty for general sale.

Tribulation, precipitous hills, bordered by low land, form the coast; but the latter cape itself consists of a lofty group, with several peaks, the highest of which is visible from the sea at twenty leagues. The heights from thence towards the north decline gradually, as the mountainous ranges approach the shore, which they join at Cape Weymouth, about latitude 12°; and from that point northward, to Cape York, the land in general is comparatively low, nor do any detached points of considerable elevation appear there. But about midway between Cape Grenville and Cape York, on the mainland south-west of Cairncross Island, a flat summit called Pudding-Pan Hill is conspicuous; and its shape, which differs from that of the hills on the east coast in general, remarkably resembles that of the mountains of the north and west coasts, to which names expressing their form have been applied*.

The line of the coast above described retires at a point which corresponds with the decline of its level; and immediately on the north of Cape Melville is thrown back to the west; so that the high land about that Cape stands out like a shoulder, more than forty miles beyond the coast-line between Princess Charlotte's Bay and the north-eastern point of Australia.

The land near Cape York is not more than four or five hundred feet high, and the islands off that point are nearly of the same elevation.

The bottom of several of the bays, on the eastern coast, not having been explored, it is still probable that rivers, or considerable mountain streams, may exist there.

Along this eastern line of shore, granite has been found throughout a space of nearly five hundred miles;—at Cape Cleveland;—Cape Grafton;—Endeavour River;—Lizard Island;—and at Clack's Island, on the north-west of the rocky mass which forms Cape Melville. And rocks of the trap formation have been obtained in three detached points among the islands off the shore;—in the Percy Isles, about latitude 21° 40′;—Sunday Island, north of Cape Grenville, about latitude 12°; and in Good's Island, on the north-west of Cape York, latitude 10° 34′.

The Gulf of Carpentaria having been fully examined by Captain Flinders, was not visited by Captain King; but the following account has been deduced from the voyage and charts of the former, combined with the specimens collected by Mr. Brown, who has also favoured me with an extract from the notes taken by himself on that part of the coast.

* *Jane's Table Land*, south-east of Princess Charlotte's Bay, (about lat. 14° 30′),—and *Mount Adolphus*, in one of the islands (about lat. 10° 40′) off Cape York, have also flat summits.—King MSS.

The

The land, on the east and south of the Gulf of Carpentaria, is so low, that for a space of nearly six hundred miles,—from Endeavour Strait to a range of hills on the mainland, west of Wellesley Islands, at the bottom of the gulf,—no part of the coast is higher than a ship's mast-head*. Some of the land in Wellesley Islands is higher than the main; but the largest island is, probably, not more than one hundred and fifty feet in height †; and low-wooded hills occur on the mainland, from thence to Sir Edward Pellew's group.—The rock observed on the shore at Coen River, the only point on the eastern side of the Gulf where Captain Flinders landed, was calcareous sandstone of recent concretional formation.

In Sweer's Island, one of Wellesley's Isles, a hill of about fifty or sixty feet in height was covered with a sandy calcareous stone, having the appearance of 'concretions rising irregularly about a foot above the general surface, without any distinct ramifications.' The specimens from this place have evidently the structure of stalactites, which seem to have been formed in sand; and the reddish carbonate of lime, by which the sand has been agglutinated, is of the same character with that of the west coast, where a similar concreted limestone occurs in great abundance.

The western shore of the Gulf of Carpentaria is somewhat higher, and from Limmen's Bight to the latitude of Groote Eylandt, is lined by a range of low hills. On the north of the latter place, the coast becomes irregular and broken; the base of the country apparently consisting of primitive rocks, and the upper part of the hills of a reddish sandstone;—some of the specimens of which are identical with that which occurs at Goulburn and Sims Islands on the north coast, and is very widely distributed on the north-west. The shore at the bottom of Melville Bay is stated by Captain Flinders to consist of low cliffs of pipe-clay, for a space of about eight miles in extent from east to west; and similar cliffs of pipe-clay are described as occurring at Goulburn Islands (see the Plate, vol. i. p. 66), and at Lethbridge Bay, on the north of Melville Island; both of which places are considerably to the west of the Gulf of Carpentaria.

Morgan's Island, a small islet in Blue-Mud Bay, on the north-west of Groote Eylandt, is composed of clink-stone; and other rocks of the trap-formation occur in several places on this coast.

The north of Blue-Mud Bay has furnished also specimens of ancient sandstone; with columnar rocks, probably of clink-stone. Round Hill, near Point Grindall, a promontory on the

* Flinders's Charts, Plate XIV.

† Flinders, vol. ii. p. 158.

north of Morgan's Island, is composed, at the base, of granite; and Mount Caledon, on the west side of Caledon Bay, seems likewise to consist of that rock, as does also Melville Island. This part of the coast has afforded the ferruginous oxide of manganese: and brown hematite is found hereabouts in considerable quantity, on the shore at the base of the cliffs; forming the cement of a breccia, which contains fragments of sand-stone, and in which the ferruginous matter appears to be of very recent production;—resembling, perhaps, the hematite observed at Edinburgh by Professor Jameson, around cast-iron pipes which had lain for some time in sand*.

The general range of the coast, it will be observed, from Limmen's Bight to Cape Arnhem, is from south-west to north-east; and three conspicuous ranges of islands on the north-western entrance of the Gulf of Carpentaria, the appearance of which is so remarkable as to have attracted the attention of Captain Flinders†, have the same general direction: a fact which is probably not unconnected with the general structure of the country. The prevailing rock in all these islands appears to be sand-stone.

The line of the main coast from Point Dale to the bottom of Castlereagh Bay, where Captain King's survey was resumed, has also a direction from south-west to north-east, parallel to that of the ranges of islands just mentioned. The low land near the north coast in Castlereagh Bay, and from thence to Goulburn Islands, is intersected by one of the few rivers yet discovered in this part of Australia,—a tortuous and shallow stream, named Liverpool River, which has been traced inland to about forty miles from the coast, through a country not more than three feet in general elevation above high-water mark; the banks being low and muddy, and thickly wooded: And this description is applicable also to the Alligator Rivers on the south-east of Van Diemen's Gulf, and to the surrounding country. The outline of the Wellington Hills, however, on the mainland between the Liverpool and Alligator Rivers, is jagged and irregular; this range being thus remarkably contrasted with the flat summits which appear to be very numerous on the north-western coast.

The specimens from Goulburn Islands consist of reddish sand-stone, not to be distinguished from that which occurs beneath the coal formation in England. On the west of these islands the coast is more broken, and the outline is irregular: but the elevation is inconsiderable; the general height in Cobourg Peninsula not being above one hundred and fifty feet

* Edin. Phil. Journ., July, 1825, p. 193.

† Flinders, vol. ii. p. 158.—See hereafter.

above

above the sea, and that of the hills not more than from three to four hundred feet.

On this part of the coast, several hills are remarkable for the flatness of their tops; and the general outline of many of the islands, as seen on the horizon, is very striking and peculiar. Thus Mount Bedwell and Mount Roe, on the south of Cobourg Peninsula; Luxmoore Head, at the west end of Melville Island; the Barthelemy Hills, south of Cape Ford; Mount Goodwin, south of Port Keats; Mount Cockburn, and several of the hills adjacent to Cambridge Gulf,—the names given to which during the progress of the survey sufficiently indicate their form, as *House-roofed*, *Bastion*, *Flat-top*, and *Square-top Hills*;—Mount Casuarina, about forty miles north-west of Cambridge Gulf;—a hill near Cape Voltaire;—Steep-Head, Port Warrender;—and several of the islands off that port, York Sound, and Prince-Regent's River;—Cape Cuvier, about latitude 24°;—and, still further south, the whole of Moresby's flat-topped Range,—are all distinguished by their linear and nearly horizontal outlines; and except in a few instances, as Mount Cockburn, Steep-Head, Mounts Trafalgar and Waterloo, (which look more like hills of flœtz-trap,) they have very much the aspect of the summits in the coal formation*.

The subjoined sketch (Pl. I. fig. 1 and 2) of some of the islands off Admiralty Gulf, (looking southward from the north-east end of Cassini Island, about lat. 13° 50′, E. long. 125° 50′,) has some resemblance to one of the views in Péron's Atlas (Pl. vi. fig. 7):—and the outline of the Iles Forbin (Pl. viii. fig. 5, of the same series, or fig. 3 in our Plate,), also exhibits remarkably the peculiar form represented in several of Captain King's drawings.

The red colour of the cliffs on the north-west and west coasts, is also an appearance which is frequently noticed on the sketches taken by Captain King and his officers. This is conspicuous in the neighbourhood of Cape Croker;—at Darch Island and Palm Bay; at Point Annesley and Point Coombe in Mountnorris Bay;—in the land about Cape Van Diemen, and on the north-west of Bathurst Island. The cliffs on Roe's River (Prince Frederic's Harbour), as might have been expected from the specimens, are described as of a reddish colour; Cape Lévêque is of the same hue; and the northern limit of Shark's Bay, (Cape Cuvier of the French,) lat. 24° 13′, which is like an enormous bastion, may be distinguished at a considerable distance by its full red colour†.

* Captain King, however, has informed me, that in some of these cases, the shape of the hill is really that of a roof, or hay-rick; the transverse section being angular, and the horizontal top an edge. † Freycinet, p. 195.

It is on the bank of the channel which separates Bathurst and Melville Islands, near the north-eastern extremity of New Holland, that a new colony has recently been established: (see Captain King's Narrative, vol. ii. p. 233.) A permanent station under the superintendence of a British officer, in a country so very little known, and in a situation so remote from any other English settlement, affords an opportunity of collecting objects of natural history, and of illustrating various points of great interest to physical geography and meteorology, which it is to be hoped will not be neglected. And as a very instructive collection, for the general purposes of geology, can readily be obtained in such situations, by attending to a few precautions, I have thought that some brief directions on this subject would not be out of place in the present publication; and have subjoined them to the list of specimens at the close of this paper.

In the vicinity of Cambridge Gulf, Captain King states, the character of the country is entirely changed;—and irregular ranges of detached rocky hills composed of sand-stone, rising abruptly from extensive plains of low level land, supersede the low and woody coast, that occupies almost uninterruptedly the space between this inlet and Cape Wessell, a distance of more than six hundred miles. Cambridge Gulf, which is nothing more than a swampy arm of the sea, extends to about eighty miles inland, in a southern direction: and all the specimens from its vicinity precisely resemble the older sandstones of the confines of England and Wales *. The View, (vol. i. Plate, p. 301,) represents in the distance Mount Cockburn, at the head of Cambridge Gulf; the flat rocky top of which was supposed to consist of sand-stone, but has also the aspect of the trap-formation. The strata in Lacrosse Island, at the entrance of the gulf, rise toward the north-west,—at an angle of about 30° with the horizon: their direction consequently being from north-east to south-west.

From hence to Cape Londonderry, towards the south, is an uniform coast of moderate elevation; and from that point to Cape Lévêque, although the outline may be in a general view considered as ranging from north-east to south-west †, the coast is remarkably indented, and the adjoining sea irregularly

* I use the term 'Old Red Sand Stone,' in the acceptation of Messrs. Buckland and Conybeare, "Observations on the South Western Coal District of England." Geol. Trans. 2nd Series, vol. i.—Captain King's specimens from Lacrosse Island are not to be distinguished from the slaty strata of that formation, in the banks of the Avon, about two miles below Clifton.

† The large chart (Sheet V.) best shows the general range of the shore, from the islands filling up the inlets.

studded

studded with very numerous islands. The specimens from this tract consist almost entirely of sand-stone, resembling that of Cambridge Gulf, Goulburn Island, and the Gulf of Carpentaria; with which the trap-formation appears to be associated.

York Sound, one of the principal inlets on this part of the coast, is bounded by precipitous rocks, from one to two hundred feet in height; and some conical rocky peaks, which not improbably consist of quartz-rock, were noticed on the eastern side of the entrance. An unpublished sketch, by Captain King, shows that the banks of Hunter's River, one of the branches of York Sound, at seven or eight miles from its opening, are composed of sand-stone, in beds of great regularity; and this place is also remarkable for a copious spring of freshwater, one of the rarest phænomena of these thirsty and inhospitable shores*.

The most considerable inlet, however, which has yet been discovered in this quarter of Australia, is Prince-Regent's River, about thirty miles to the south-west of York Sound,—the course of which is almost rectilinear for about fifty miles in a south-eastern direction; a fact which will probably be found to be connected with the geological structure of the country. The general character of the banks, which are lofty and abrupt, is precisely the same with that of the rivers falling into York Sound; and the level of the country does not appear to be higher in the interior than near the coast. The banks are from two to four hundred feet in height, and consist of close-grained siliceous sand-stone, of a reddish hue †; and the view, (Plate, vol. ii. p. 46,) shows that the beds are nearly horizontal, and very regularly disposed; the cascade there represented being about one hundred and sixty feet in height, and the beds from six to twelve feet in thickness. Two conspicuous hills, which Captain King has named Mounts Trafalgar and Waterloo, on the north-east of Prince-Regent's River, not far from its entrance, are remarkable for cap-like summits, much resembling those which characterize the trap-formation. (See fig. 5.)

The coast on the south of this remarkable river, to Cape Lévêque, has not yet been thoroughly examined; but it appears from Captain King's Chart (No. V.) to be intersected by several inlets of considerable size, to trace which to their termination is still a point of great interest in the physical geography of New Holland. The space thus left to be explored from the Champagny Isles to Cape Lévêque, corresponds to more than one hundred miles in a direct line; within which extent nothing but islands and detached portions of land have

* Narrative, i. p. 405.

† Narrative, i. pp. 434-437, and ii. p. 45.

yet been observed. One large inlet especially, on the south-east of Cape Lévêque, appears to afford considerable promise of a river; and the rise of the tide within the Buccaneer's Archipelago, where there is another unexplored opening, is no less than thirty-seven feet.

The outline of the coast about Cape Lévêque itself is low, waving, and rounded; and the hue for which the cliffs are remarkable in so many parts of the coast to the north, is also observable here, the colour of the rocks at Point Coulomb being of a deep red:—but on the south of the high ground near that Point, the rugged stony cliffs are succeeded by a long tract, which to the French voyagers (for it was not examined by Captain King,) appeared to consist of low and sandy land, fronted by extensive shoals. It has hitherto been seen, however, only at a distance; so that a space of more than three hundred miles, from Point Gantheaume nearly to Cape Lambert, still remains to be accurately surveyed.

Depuch Island, east of Dampier's Archipelago, about latitude 20° 30′, is described by the French naturalists as consisting in a great measure of columnar rocks, which they supposed to be *volcanic;* and they found reason to believe that the adjoining continent was of the same materials*. It is not improbable, however, that this term was applied to columns belonging to the trap formation, since no burning mountain has been any where observed on the coast of New Holland:—nor do the drawings of Depuch Island, made on board Captain King's vessel, give reason to suppose that it is at present eruptive. Captain King's specimens from Malus Island, in Dampier's Archipelago, (sixty miles further west) consist of green-stone and amygdaloid.

The coast is again broken and rugged about Dampier's Archipelago, latitude 20° 30′; and on the south of Cape Preston, in latitude 21°, is an opening of about fifteen miles in width, between rocky hills, which has not been explored. From thence to the bottom of Exmouth Gulf,—more than one hundred and fifty miles, the coast is low and sandy, and does not exhibit any prominences. The west coast of Exmouth Gulf itself is formed by a promontory of level land, terminating in the North-west Cape; and from thence to the south-west, as far as Cape Cuvier, the general height of the coast is from four to five hundred feet; nor are any mountains visible over the coast range.

Several portions of the shore beween Shark's Bay and Cape Naturaliste have been described in the account of Commodore Baudin's expedition; but some parts still remain to be surveyed.

* Péron, vol. i. p. 130.

veyed. From the specimens collected by Captain King and the French descriptions, it appears that the islands on the west of Shark's Bay abound in a concretional calcareous rock of very recent formation, similar to what is found on the shore in several other parts of New Holland, especially in the neighbourhood of King George's Sound;—and which is abundant also on the coast of the West Indian Islands, and of the Mediterranean. Captain King's specimens of this production are from Dirk Hartog's and Rottnest Islands; and M. Péron states that the upper parts of Bernier and Dorre Islands are composed of a rock of the same nature. This part of the coast is covered in various places with extensive dunes of sand; but the nature of the base, on which both these and the calcareous formation repose, has not been ascertained.

The general direction of the rocky shore, from North-west Cape to Dirk Hartog's Island, is from the east of north to the west of south. On the south of the latter place the land turns towards the east. High, rocky and reddish cliffs have been seen indistinctly about latitude 27°; and a coast of the same aspect has been surveyed, from Red Point, about latitude 28°, for more than eighty miles to the south-west. The hills called Moresby's flat-topped Range, of which Mount Fairfax, latitude 28° 45′, is the highest point, occupy a space of more than fifty miles from north to south.

Rottnest Island and its vicinity, latitude 32°, contains in abundance the calcareous concretions already mentioned; which seem there to consist in a great measure of the remains of recent shells, in considerable variety. The islands of this part of the shore have been described by MM. Péron and Freycinet*; and the coast to the south, down to Cape Leeuwin, the south-western extremity of New Holland, having been sufficiently examined by the French voyagers, was not surveyed by Captain King.

Swan River (*Rivière des Cygnes*), upon this part of the coast, latitude 31° 25′ to 32°, was examined by the French expedition, to the distance of about twenty leagues from its mouth; and found still to contain salt water. The rock in its neighbourhood consisted altogether of sandy and calcareous incrustations, in horizontal beds, inclosing, it is stated, shells and the roots and even trunks of trees. Between this river and Cape Péron, a "great bay" was left unexplored†.

The prominent mass of land, which stands out from the main, between Cape Naturaliste and Cape Leeuwin, and runs nearly on the meridian for more than fifty miles, seems to have

* Péron, vol. ii. p. 168, &c.

† Péron, vol. i. p. 179. Freycinet, p. 5. 170.

a base

a base of granite, which, at Cape Naturaliste, is said to be stratified *. The same rock also occurs, among Captain King's specimens, from Bald-head in King George's Sound; but nearly on the summit of that hill, which is about five hundred feet high, were found the ramified calcareous concretions, erroneously considered as corals by Vancouver and others †; but which appear, from Captain King's specimens, to be nothing more than a variety of the recent limestone so abundant throughout these shores.

The south coast, and the southern portion of the east coast of Australia, which were surveyed by Captain Flinders, are described in the account of his voyage, and do not come within the object of the present paper.

Geological Remarks.

I. The rocks, of which specimens occur in the collections of Captain King and Mr. Brown, are the following:

Granite	Cape Cleveland; C. Grafton; Endeavour River; Lizard I.; Round Hill, near C. Grindall; Mount Caledon; Island near C. Arnhem; Melville Bay; Bald-head, K. George's Sound.
Various Slaty Rocks,	
Mica-Slate	Mallison's I.
Talc-Slate	Endeavour River.
Slaty Clay	Inglis's I., Clack I., Percy I.
Hornblende Rock?	Pobassoo's Island; Half-way Bay, P.-Regent's River.
Granular Quartz	Endeavour River; Montagu-Sound, North-west Coast.
Epidote	C. Clinton?; Port Warrender; Careening Bay.
Quartzose Conglomerates, and *ancient Sandstones*	Rodd's Bay; Islands of the north and north-west Coasts; Cambridge Gulf; York Sound; P.-Regent's River.
Pipe-clay	Melville Bay; Goulburn I.; Lethbridge Bay.

Rocks of the Trap Formation.

Serpentine	Port Macquarie; Percy Isles.

* Péron, vol. i. p. 69.

‡ Vancouver, i. 49. D'Entrecasteaux, ii. 175. Freycinet, 105. Flinders, i. 63.—See the Detailed Descriptions, hereafter; and Captain King's Narrative, vol. i. p. 12.

Sienite

Sienite	Rodd's Bay.
Porphyry	C. Cleveland.
Porphyritic Conglomerate .	C. Clinton, Percy I., Good's I.
Compact Felspar	Percy I., Repulse Bay, Sunday I.
Green-stone	Vansittart Bay, Bat I., Careening Bay, Malus I.
Clink-stone	Morgan's I., Pobassoo's I.
Amygdaloid, with Calcedony	Port Warrender; Half-way Bay; Bat I.; Malus I.
Wacke?	Bat Island.

Recent calcareous Breccia .	Sweer's I., N. coast.—Dirk Hartog's and Rottnest I., &c., W. coast.—King George's Sound, S. coast.

The only information that has been published respecting the geology of New Holland, besides what is contained in the Voyages of Captain Flinders and Commodore Baudin, is a slight notice, by Professor Buckland, of some specimens collected during Mr. Oxley's Expedition to the River Macquarie*, in 1818; and a brief outline of a paper by the Rev. Archdeacon Scott, entitled "A Sketch of the Geology of New South Wales and Van Diemen's Land," which has been read before the Geological Society†. On these authorities, the following may be added to the preceding list of rocks:—

Limestone,—resembling in the character of its organic remains the mountain limestone of England. . . .	Interior of New Holland, near the east coast; Van Diemen's Land, (Buckland; Prevost MSS; Scott).
The Coal-formation	East coast of New Holland; Van Diemen's Land.—(Buckland—Scott.)
Indications of *the new red-Sandstone* (Red-Marl), afforded by the occurrence of *Salt*	Van Diemen's Land. (Scott.)
Oolite	Van Diemen's Land. (Scott.)

II. The specimens of Captain King's and Mr. Brown's collections, without any exception, agree with those of the same

* Geol. Trans. vol. v. p. 480.

† Ann. of Phil. June, 1824. I am informed that Mr. Von Buch also has published a paper on the rocks of New Holland; but have not been so fortunate as to meet with it.

[Dr. Fitton here subjoins an extract from the Report on M. Duperrey's Voyage, in order to complete the catalogue of the Australian rocks, according to the present state of our information. Having, however, given the whole of this Report in our last volume, we shall refer the reader to Phil. Mag. vol. lxvii. p. 364, for the statement in question.—Edit.]

 denomi-

denominations from other parts of the world; and the resemblance is, in some instances, very remarkable:—The sandstones of the west and north-west of New Holland are so like those of the west of England, and of Wales, that the specimens from the two countries can scarcely be distinguished from each other; the arenaceous cement in the calcareous breccia of the west coast is precisely the same with that of Sicily; and the jasper, calcedony, and green quartz approaching to heliotrope, from the entrance of Prince-Regent's River, resemble those of the Tyrol, both in their characters and association.—The Epidote of Port Warrender and Careening Bay, affords an additional proof of the general distribution of that mineral; which, though perhaps it may not constitute large masses, seems to be of more frequent occurrence as a component of rocks than has hitherto been supposed *. The mineral itself, both crystallized and compact, the latter in the form of veins traversing sienitic rocks, occurs, in Mr. Greenough's cabinet alone,—from Malvern, North Wales, Ireland, France, and Upper Saxony. Mr. Kœnig has found it extensively in the sienitic tract of Jersey †; where blocks of a pudding-stone, bearing some resemblance to the green breccia of Egypt, were found to be composed of compact epidote, including very large pebbles of a porphyritic rock, which itself contains a considerable proportion of this substance.—And Mr. Greenough has recently received, among specimens sent home by Mr. J. Burton, junior, a mass of compact epidote, with quartz and felspar, from Dokhan, in the desert between the Red Sea and the Nile. When New Holland is added to these localities, it will appear that few minerals are more widely diffused.

III. The unpublished sketches, by Captain King and Mr. Roe, of the hills in sight during the progress of the survey of the Coasts of Australia, accord in a very striking manner with the geological character of the shore. Those from the east coast, where the rocks are primitive, representing strongly marked and irregular outlines of lofty mountains, and frequently, in the nearer ground, masses of strata highly inclined. The outlines on the contrary, on the north, north-west, and western shores, are most commonly uniform, rectilinear,—the summits flat, and diversified only by occasional detached and conical peaks, none of which are very lofty.

IV. No information has yet been obtained, from any of the collections, respecting the diluvial deposits of Australia: a class of phænomena which is of the highest interest, in an island of such vast extent, so very remote in situation, and of

* See Cleaveland's Mineralogy, 1816, p. 297-300.

† Plee's Account of Jersey, 4to. Southampton, 1817. p. 231-276.

which

which the existing animals are so different from those of other parts of the globe. It is remarkable, also, that no lime-stone is among the specimens from the northern and western shores, except that of the recent breccia; and although negative conclusions are hazardous, it would seem probable, from this circumstance, that lime-stone cannot be very abundant or conspicuous at the places visited.—No eruptive mountains, nor any traces of recent volcanic eruption, have yet been observed in any part of Australia.

V. The recent calcareous breccia, of which a description will be found in the "Detailed list of specimens," is one of the most remarkable productions of New Holland. It was found, during the expedition of Commodore Baudin, to exist throughout a space of no less than twenty-five degrees of latitude, and an equal extent of longitude, on the southern, west, and north-west coasts*; and from Mr. Brown's specimens it appears to occur also on the shores of the Gulf of Carpentaria. The full account which M. Péron has given of this formation, sufficiently shows its resemblance to the very recent lime-stone, full of marine shells, which abounds on the shores of the Mediterranean, the West India Islands, and in several other parts of the world: And it is a point of the greatest interest in geology, to determine, whether any distinct line can really be drawn, between those concretions, unquestionably of modern formation, which occur immediately upon the shore: and other calcareous accumulations, very nearly resembling them, if not identical, both in the fossils they contain, and in the characters of the cementing substances, that are found in several countries, at considerable heights above the sea.

Dr. Buckland has described a breccia of modern formation, which occurs upon the shore at Madagascar, and consists of a firmly-compacted cream-coloured stone, composed of granular fragments of shells, agglutinated by a calcareous cement †. The stone of Guadaloupe, containing the human skeletons, is likewise of the same nature; and its very recent production cannot be doubted, since it contains fragments of stone axes, and of pottery ‡.—The cemented shells of Bermuda, described by Captain Vetch §, which pass gradually into a compact limestone, differ only in colour from the Guadaloupe stone; and agree with it, and with the calcareous breccia of Dirk Hartog's Island, in the gradual melting down of the cement into the included portions, which is one of the most remarkable features

* Voyage, ii. p. 168, 169—216, &c.

† Geol. Trans. vol. v. p. 479.

‡ Linnean Trans. xii. p. 53—57.

§ Geol. Trans. 2d Series, vol. i. p. 172.

of that rock*. A calcareous compound, apparently of the same kind, has been recently mentioned, as of daily production in Anastasia Island, on the coast of East Florida†; and will probably be found to be of very general occurrence in that quarter of the globe. And Captain Beaufort's account of the process by which the gravelly beach is cemented into stone, at Selinti, and several other places on the coast of Karamania, on the north-east of the Mediterranean‡, accords with M. Péron's description of the progress from the loose and moveable sands of the dunes to solid masses of rock§. In the island of Rhodes, also, there are hills of pudding-stone, of the same character, considerably elevated above the sea. And Captain W. H. Smyth, the author of Travels in Sicily, and of the Survey of the Mediterranean recently published by the Admiralty, informs me, that he has seen these concretions in Calabria, and on the coasts of the Adriatic;—but still more remarkably in the narrow strip of recent land, (called the Placca,) which connects Leucadia, one of the Ionian Islands, with the continent, and so much resembles a work of art, that it has been considered as a Roman fabric. The stone composing this isthmus is so compact, that the best mill-stones in the Ionian Islands are made from it; but it is in fact nothing more than gravel and sand cemented by calcareous matter, the accretion of which is supposed to be rapidly advancing at the present day.

The nearest approach to the concreted sand-rock of Australia, that I have seen, is in the specimens presented by Dr. Daubeny to the Bristol Institution, to accompany his excellent paper on the geology of Sicily ||; which prove that the arenaceous breccia of New Holland is very like that which occupies a great part of the coast, almost entirely around that island. Some of Dr. Daubeny's specimens from Monte Calogero, above Sciacca, consist of a breccia, containing angular fragments of splintery limestone, united by a cement, composed of minute grains of quartzose-sand disseminated in a calcareous paste, resembling precisely that of the breccia of

* Kœnig. Phil. Trans. 1814, p. 107, &c.

† *Bulletin des Sciences Nat. Mars*, 1825.

‡ Beaufort's "Description of the South Coast of Asia Minor," &c. Second edition. London, 1818: pp. 180—184, &c. In the neighbourhood of Adalia, the deposition of calcareous matter from the water, is so copious, that an old water-course had actually 'crept upwards to a height of nearly three freet; and the rapidity of the deposition was such, that some specimens were collected on the grass, where the stony crust was already formed, although the verdure of the leaf was as yet but imperfectly withered (p. 114): a fact, which renders less extraordinary M. Péron's statement, that the excrements of kangaroos had been found concreted by calcareous matter.—Péron, vol. ii. p. 116. § Voyage, ii. 116.

|| Edin. Phil. Journ. 1825, pp. 116, 117, 118, and 254-5.

Dirk

Dirk Hartog's Island: and a compound of this kind, replete with shells, not far, if at all, different from existing species, fills up the hollows in most of the older rocks of Sicily; and is described as occurring, in several places, at very considerable heights above the sea. Thus, near Palermo, it constitutes hills some hundred feet in height;—near Girgenti, all the most elevated spots are crowned with a loose stratum of the same kind; and the heights near Castrò Giovanni, said to be 2880 feet above the sea, are probably composed of it. But although the concretions of the interior in Sicily much resemble those of the shore, it is still doubtful whether the former be not of more ancient formation; and if they contain nummulites, they would probably be referred to the epoch of the beds within the Paris basin.

The looser breccia of Monte Pelegrino, in Sicily, is very like the less compacted fragments of shells from Bermuda, described by Captain Vetch, and already referred to*:—and the rock in both these cases, nearly approaches to some of the coarser oolites of England.

The resemblance pointed out by M. Prevost†, of the specimens of recent breccia from New Holland, in the museum at the Jardin du Roi, to those of St. Hospice near Nice, is confirmed by the detail given by Mr. Allan in his sketch of the geology of that neighbourhood ‡; in which the perfect preservation of the shells, and their near approach to those of the adjoining sea at the present day, are particularly mentioned; and it is inferred that the date of the deposit which affords them, is anterior to that of the conglomerate containing the bones of extinct quadrupeds, likewise found in that country. M. Brongniart also, who examined the place himself, mentions the recent accumulation 'which occurs at St. Hospice, about sixty feet above the present level of the sea,' as containing marine shells in a scarcely fossile state, ('*à peine fossiles*;') and he describes the mass in which they occur, as belonging to "a formation still more recent than the upper marine beds of the environs of Paris§."

The geological period indicated by these facts, being probably more recent than the tertiary beds containing nummulites, and generally than the Paris and London strata, accords with the date which has hitherto been assigned to the 'crag' beds of Suffolk, Essex, and Norfolk ||: but later observations render

* These specimens are in the Museum of the Geological Society.

† Prevost MSS. See Detailed list of specimens.

‡ Trans. of the R. Soc. of Edinb. vol. viii. 1818, p. 427, &c.—See also the previous publications of M. Risso. *Journal des Mines*, tom. xxxiv. &c.

§ Brongniart, in Cuvier's *Ossemens Fossiles*, 2d. Edit. vol. ii. p. 427.

|| Conybeare and Phillips' "Outlines," &c. p. 11.—Geol. Trans. i. p. 327, &c.—

render doubtful the opinion generally received respecting the age of these remarkable deposits, and a full and satisfactory account of them is still a desideratum in the geology of England.—When, also, our imperfect acquaintance with the travertino of Italy, and other very modern lime-stones containing fresh-water shells, is considered*,—the continual deposition of which, at the present time, cannot be questioned, (though probably the greater part of the masses which consist of them may belong to an æra preceding the actual condition of the earth's surface),—it would seem that the whole subject of these newer calcareous formations requires elucidation: and, if the inferences connected with them do not throw considerable doubt upon some opinions at present generally received, they show, at least, that a great deal more is to be learned respecting the operations and products of the most recent geological epochs, than is commonly supposed.

Since it appears that the accretion of calcareous matter is continually going on at the present time, and has probably taken place at all times, the stone thus formed, independent of the organized bodies which it envelopes, will afford no criterion of its date,—nor give any very certain clue to the revolutions which have subsequently acted upon it. But as *marine* shells are found in the cemented masses, at heights above the sea, to which no ordinary natural operations could have conveyed them, the elevation of these shells to their actual place, (if not that of the rock in which they are agglutinated,) must be referred to some other agency:—while the perfect preservation of the shells, their great quantity, and the abundance of the same species in the same places, make it more probable that they lay originally in the situations where we now find them, than that they have been transported from any considerable distances, or elevated by any very turbulent operation. Captain de Freycinet, indeed, mentions that patellæ, worn by attrition, and other recent shells, have been found on the west

&c.—Taylor in Geol. Trans. 2d series, vol. ii. p. 371. Mr. Taylor states the important fact that 'the remains of unknown animals are buried together with the shells' in the crag of Suffolk; but does not mention the nature of these remains.—Since these pages have been at the press, Mr. Warburton, by whom the coast of Essex and Norfolk has been examined with great accuracy, has informed me, that the fossil bones of the crag, are the same with those of the diluvial gravel,—including the remains of the elephant, rhinoceros, stag, &c.

* Some valuable observations on the formation of recent limestone, in beds of shelly marl at the bottom of lakes in Scotland, have been read before the Geological Society, by Mr. Lyell, and will appear in the volume of the Transactions now in the press.—See Annals of Philosophy, 1825, p. 310.

coast

coast of New Holland, on the top of a wall of rocks an hundred feet above the sea,—evidently brought up by the surge during violent storms*; but such shells are found in the breccia of Sicily, and in several other places, at heights too great, and their preservation is too perfect, to admit of this mode of conveyance; and to account for their existence in such situations, recourse must be had to more powerful means of transport.

The occurrence of corals, and marine shells of recent appearance, at considerable heights above the sea, on the coasts of New Holland, Timor, and several other islands of the south, was justly considered by M. Péron as demonstrating the former "abode of the sea" above the land; and very naturally suggested an inquiry, as to the nature of the revolutions to which this change of situation is to be ascribed†. From similar appearances at Pulo Nias, one of the islands off the western coast of Sumatra, Dr. Jack also was led to infer, that the "surface of that island must at one time have been the bed of the ocean;" and after stating, "that by whatever means it obtained its present elevation, the transition must have been effected with little violence or disturbance to the marine productions at the surface‡," he concludes, that the phænomena are in favour of an "*heaving up of the land, by a force from beneath.*" The probable nature of this force is indicated most distinctly, if not demonstrated, by the phænomena which attended the memorable earthquake of Chili, in November 1820§, which was felt throughout a space of fifteen hundred miles from north to south. For it is stated upon the clearest evidence, that after formidable shocks of earthquake, repeated with little interruption during the whole night of the 19th of November (and the shocks were continued afterwards, at intervals, for several months), "*it appeared*, on the morning of

* Freycinet, p. 187.—The presence of shells in such situations may often be ascribed to the birds, which feed on their inhabitants. At Madeira, where recent shells are found near the coast at a considerable height above the sea, the gulls have been seen carrying up the living patellæ, just taken from the rocks.

† Péron, Voyage, &c. vol. ii. pp. 165-183.

‡ Geol. Trans. second series, vol. i. p. 403, 404.

§ The statements here referred to, are those of Mrs. Graham, in a letter to Mr. Warburton, which has been published in the Geological Transactions (second series, vol. i. p. 412, &c.); and the account is supported and illustrated by a valuable paper in the Journal of the Royal Institution for April 1824 (vol. xvii. p. 38, &c.). The writer of this latter article asserts, that "the whole country, from the foot of the Andes to far out at sea, was raised by the earthquake; the greatest rise being at the distance of about two miles from the shore. The rise upon the coast was from two to four feet:—at the distance of a mile, inland, it must have been from five to six, or seven feet." pp. 40, 45.

the

the 20th, *that the whole line of coast, from north to south, to a distance of about one hundred miles, had been raised above its former level.*"—" The alteration of level at Valparaiso was about three feet; and some rocks were thus newly exposed, on which the fishermen collected the scallop-shell fish, which was not known to exist there before the earthquake. At Quintero the elevation was about four feet.—" When I went," the narrator adds, " to examine the coast, although it was high-water, I found the ancient bed of the sea laid bare, and dry, with beds of oysters, muscles, and other shells adhering to the rocks on which they grew,—the fish being all dead, and exhaling most offensive effluvia. And I found good reason to believe that the coast had been raised by earthquakes at former periods in a similar manner; several ancient lines of beach, consisting *of shingle mixed with shells,* extending in a parallel direction to the shore, to the height of fifty feet above the sea."—Such an accumulation of geological evidence, from different quarters and distinct classes of phænomena, concurs to demonstrate the existence of most powerful expansive forces within the earth, —and to testify their agency in producing the actual condition of its surface,—that the phænomena just now described are nothing more than what was to be expected from previous induction. These facts, however, not only place beyond dispute the existence of such forces,—but show that, even in detail, their effects accord most satisfactorily with the predictions of theory. It is not, therefore, at all unreasonable to conceive, that, in other situations, phænomena of the same character have been produced by the same cause,—though we may not at present be enabled to trace its connexion with the existing appearances so distinctly; and though the facts, when they occurred, may have been unnoticed,—or may have taken place at periods beyond the reach of historical record, or even beyond the possibility of human testimony.

M. Péron has attributed the great abundance of the modern breccia of New Holland to the large proportion of calcareous matter, principally in the form of comminuted shells, which is diffused through the siliceous sand of the shores in that country*; and as the temperature, especially of the summer, is very high on that part of the coast where this rock has been principally found, the increased solution of carbonate of lime by the percolating water, may possibly render its formation more abundant there, than in more temperate climates. But the true theory of these concretions, under any modification of temperature, is attended with considerable difficulty:—and it is certain that the process is far from being confined to the

* Péron, Voyage, &c. ii. p. 116.

warmer

warmer latitudes. Dr. Paris has given an account of a modern formation of sand-stone on the northern coast of Cornwall*; where a large surface is covered with a calcareous sand, that becomes agglutinated into a stone, which he considers as analogous to the rocks of Guadaloupe; and of which the specimens that I have seen, resemble those presented by Captain Beaufort to the Geological Society, from the shore at Rhodes. —Dr. Paris ascribes this concretion, not to the agency of the sea, nor to an excess of carbonic acid, but to the solution of carbonate of lime itself in water, and subsequent percolation through calcareous sand; the great hardness of the stone arising from the very sparing solubility of this carbonate, and the consequently very gradual formation of the deposit.—Dr. Mac Culloch describes calcareous concretions, found in banks of sand in Perthshire, which " present a great variety of stalactitic forms, generally more or less complicated, and often exceedingly intricate and strange†," and which appear to be analogous to those of King George's Sound and Sweer's Island :—And he mentions, as not unfrequently occurring in sand, in different parts of England (the sand above the fossil bones of Norfolk is given as an example), long cylinders or tubes, composed of sand agglutinated by carbonate of lime, or 'calcareous stalactites entangling sand,' which, like the concretions of Madeira, and those taken for corals at Bald-Head, " have been ranked improperly, with organic remains."

The stone which forms the fragments in the breccia of New Holland, is very nearly the same with that of the cement by which they are united;—the difference consisting only in the greater proportion of sand which the fragments contain :—and it would seem, that after the consolidation of the former, and while the deposition of similar calcareous matter was still in progress, the portions first consolidated must have been shattered by considerable violence. But, where no such fragments exist, the unequal diffusion of components at first uniformly mixed,—and even the formation of nodules differing in proportions from the paste which surrounds them, may perhaps admit of explanation, by some process analogous to what takes place in the preparation of the compound of which the ordinary earthenware is manufactured;—where, though the ingredients are divided by mechanical attrition only, a sort of chemical action produces, under certain circumstances, a new arrangement of the parts‡. And this explanation may, pro-

* Trans. of the Geol. Soc. of Cornwall, vol. i. p. 1, &c.

† " On an arenaceo-calcareous substance," &c. — Quarterly Journal (Royal Institution), Oct. 1823, vol. xvi. p. 79-83.

‡ The clay and pulverized flints are combined for the use of the potter,

 by

bably, be extended to those nodular concretions, generally considered as contemporaneous with the paste in which they are enveloped, the distinction of which, from conglomerates of mechanical origin, forms, in many cases, a difficulty in geology. What the degree may be, of subdivision required to dispose the particles to act thus upon each other, or of fluidity to admit of their action, remains still to be determined.

[To be continued.]

V. *On Specific and Latent Heat, and on Alcoholic Engines. By Mr.* Henry Meikle *.

THERE are few subjects on which there has been a greater diversity of opinion than on the laws according to which heat is distributed in different bodies, or in the same body under different forms. To Dr. Black we are indebted for the first important discoveries on this intricate subject; and from his time, many others, following up his researches, have gradually added to our stock of knowledge. That absolute certainty is by no means yet attained on many leading points connected with this inquiry, there can be little room to doubt; and as an instance of primary importance, it need only be recollected what different opinions are still entertained regarding the scales of thermometers, or whether the influx of heat be exactly proportional to the corresponding expansion: many experiments favouring the notion, that whilst the heat increases uniformly, the expansion, in a variety of bodies, proceeds with an accelerated velocity.

It had been for some time suspected and even so far sanctioned by experiment, that under equal volumes the specific heats of the simple gases are equal; and more lately, Mr. Haycraft seems to have added considerably to the evidence in favour of the same opinion; though his mode of operating, probably the most correct yet used, seems to be still a little open to objection.

by being first separately diffused in water to the consistence of thick cream, and when mixed in due proportion are reduced to a proper consistence by evaporation. During this process, if the evaporation be not rapid and immediate, or if the ingredients are left to act on each other, even for twenty-four hours, the flinty particles unite into sandy grains, and the mass becomes unfit for the purposes of the manufacturer. I am indebted for this interesting fact, which, I believe, is well known in some of the potteries, to my friend Mr. Arthur Aikin. And Mr. Herschel informs me, that a similar change takes place in recently precipitated carbonate of copper; which, if left long moist, concretes into hard gritty grains, of a green colour, much more difficultly soluble in ammonia than the original precipitate.

* Communicated by the Author.

Mr.

Mr. Haycraft's experiments are detailed in the Transactions of the Royal Society of Edinburgh, vol. x., and in the Philosophical Magazine, vol. lxiv. His apparatus consisted principally of two equal cylinders or forcing pumps, one of which made a certain volume of heated air circulate through a pipe immersed in cold water; and the other did the same thing with an equal volume of some other gas. Their specific heats under equal volumes, were then compared, supposing them proportional to the effects they had in separately heating equal quantities of the cold water.

For the purpose of abstracting moisture from the gases, each cylinder contained some muriate of lime: but it would have been fully as satisfactory had the gases been rendered dry previously to their introduction, for this salt might again give out moisture as the temperature rose. Besides, if the absorbent substance abstracted more moisture from one gas than from the other, it would at the same time reduce its elasticity the more, and with it the capacity for heat under a given volume, which was that of the contents of the apparatus. We are not informed whether the interior parts of the apparatus were oiled or not; for if any substance capable of evaporation or of combining with the gases came into contact with them, there is reason to suspect the consequences. That the gases were somehow contaminated during the operation, appears from their being less pure at the end of the process than at the commencement, even when they ought to have been more free from moisture, after having been all the while exposed to muriate of lime.

From perusing the account of Mr. Haycraft's experiments on mixtures of gases and vapours, I am not so clear about acquiescing in the conclusions he has drawn, regarding their specific heats; as from several circumstances it is doubtful if they be sufficient to overturn the hypothesis of equal specific heats belonging to equal volumes whenever the elasticities and temperatures are equal. As to his experiments on carburetted hydrogen, I rather suspect some decomposition had taken place, by friction, change of temperature, &c.; and if such were the case, there is reason to think that both the elasticity and heat might undergo some change. It would have rendered such experiments more satisfactory, had the apparatus been provided with gauges by which the elasticities of the included gases could have been ascertained or compared at any stage of the process; for if these elasticities differed more than in the ratio of the temperatures of the calorimeters increased by 448°, there could be little doubt that all was not correct. The apparatus, it is true, might have been constructed to

 maintain

maintain an equilibrium, in some cases at least, between the elasticities of the two gases, or between these and that of the atmosphere; but this would have had its defects.

The experiments on the air of respiration are more liable to objection; for if this was introduced into the apparatus at the temperature of the lungs, it would of course be capable of holding in solution more moisture than when it afterwards had its temperature reduced by coming into contact with the colder parts of the apparatus, especially the calorimeter, where the moisture would remain condensed in a useless state*; and the air of respiration after being so deprived of its moisture would be inferior in elasticity to the common air in the other parts of the apparatus. If this suspicion be well founded, the result ought apparently to be as is stated by Mr. H.; for it is well known, that at the same temperature, the heat in a given volume of elastic fluid decreases when the tension does so; though the reverse holds of the heat in a given weight.

It may therefore be presumed that, besides what has been already suggested, several sources of inaccuracy might have been avoided, both in these experiments, and more particularly in those of MM. Delaroche and Berard, had the process been somewhat reversed, by employing gases at a temperature a little above that at which they were introduced †, to cool the water in the calorimeter previously raised to a still higher temperature.

From the above experiments, perhaps the best yet published, Mr. Haycraft endeavours to draw several practical conclusions, which, though very ingenious, scarcely appear to be all borne out with sufficient evidence. The resistance of the air to instantaneous expansion may, as he very properly suggests, augment the temperature in the firing of gunpowder; but it is doubtful if the same remark be equally applicable to the case of furnaces. The resistance to expansion can never amount to the force with which air rushes into an air furnace, and which is always inferior to the atmospheric pressure. For air rushes into a furnace in some degree as into a vacuum in a reduced state of elasticity; because it can only be from a diminution of pressure on the side next the fire that the air flows towards it at all. Combustion in a furnace, being a continued and repeated process, differs essentially from the instantaneous and single act of explosion in gunpowder. The continued upward

* It is true, that any vapour on condensing, gives out its latent heat; but in the present case, this would not be likely to compensate for the other defect of its continuing dormant in a liquid state during the whole process.

† For moisture in a state of saturation often adheres to a dull or oxidized surface not colder than itself.

discharge

discharge of heat from a furnace by means of the highly rarified ascending gases, seems to render any thing like resistance to instantaneous expansion unnecessary and improbable.

Mr. Haycraft, speaking of combustion, says, "This formation [of carbonic acid] does not consist of a conversion of oxygen into carbonic acid, but of a union of two ingredients into a compound having an *absolute* capacity for caloric equal to one of the ingredients only; namely, the oxygen gas: consequently the whole absolute heat of the carbon is rendered free." Now on this it need only be remarked, that Mr. Haycraft seems to have entirely forgotten that we are still totally in the dark as to the *absolute* quantity of heat in bodies. Indeed, we do not even know which of two bodies of different compositions contains most heat; and whilst this is the case, it is no wonder that we are ignorant of their exact proportions. Mr. Haycraft has, no doubt, at great trouble and expense rendered an important service to science, by showing in a more satisfactory way than had been done, that, under equal volumes, the *specific* heats of the gases are equal: but we are not therefore warranted to conclude that their *absolute* heats are equal. That a knowledge of the specific heats throws little or no light on the absolute quantity, is plain from the circumstance, that steam, though inferior to water in the former quality, much exceeds it in the latter. If such be the case with the same body under different forms,—how much more so of different bodies?

As steam is one of the most useful mechanical agents, its powers are deserving of minute examination. They have long engaged the attention both of our own countrymen and of foreigners; and on some of their opinions and experiments I now take the liberty of making a few remarks.

On opening a pipe connected with a steam-boiler and condensing a certain weight of the vapour in a vessel of cold water, M. Clement found that the increase in the water's temperature was independent of the elasticity of steam in the boiler; at least it was sensibly so whilst that elasticity was equal, double or triple the atmospheric pressure. From this he inferred, that the entire heat in a given weight of steam in a state of saturation must be the same at all temperatures. But this conclusion, though pretty generally acquiesced in, has been rather too hastily drawn to bear examination. It is well known, that an elastic fluid when allowed to expand without any accession of heat will be reduced in its temperature; but if at the same time in contact with a hotter body, or one which possesses its former temperature, it will readily absorb heat from that body. When, therefore, the steam which possessed

double

double the atmospheric tension, issued forth against only half that force, it must have expanded considerably; and consequently, without some additional heat, its temperature would have been much reduced; but being at the same time in contact with the stop-cock and pipe, which were still about 248°, there can be no doubt that these communicated heat to it; and therefore, if on condensing in the bucket, this vapour did not heat the water more than an equal weight of steam of half the original elasticity did, the obvious conclusion is, that in the boiler the same weight of denser steam contained less heat*.

But this and several other sources of fallacy are deserving of more particular consideration. On account of the high pressure, the aperture through which the steam issued must have been very contracted; probably, it was but a small part only of the circular hole in the stopper of the stop-cock; and therefore there was so much the greater chance of the expanding vapour absorbing heat from its contact with the hotter metal; especially when we consider that the vapour would enter by such a small opening into the cylindrical cavity of the stopper, where it would have an opportunity of expanding and absorbing heat; and after dashing from side to side of this little chamber would pass out of it by a like narrow aperture diagonally opposite to that by which it entered. On passing this second orifice, it would still further expand and absorb more heat from the hot metallic pipe. Besides, if the velocity of the steam was not ultimately the same in all the three cases, it is reasonable to think, that its elasticity, whilst rushing through the pipe, would, in the high-pressure cases, be even inferior to that of the atmosphere; otherwise, its momentum, which is independent of refrigeration, would have been sufficient to drive all the water out of the bucket. I should therefore suppose, that if the velocities were greater in the higher temperatures, still the elasticity and momentum taken together, could but little more than balance the atmospheric pressure. If this notion be correct, it will follow that the greater the elasticity and temperature within the boiler, the less would these be in the stream of vapour; and consequently

* It has often been observed, that steam issuing from a pipe at 212° is quite transparent till at some distance from the pipe; but that a stream of high-pressure steam issues opaque from the very orifice. This shows incontrovertibly that a given weight of the high-pressure steam had contained less heat than a like quantity of the common sort. Otherwise, such heat would just have enabled it to maintain the transparent elastic form when it expanded into steam of the ordinary atmospheric pressure. This remark is somewhat at variance with M. Clement's result; and perhaps the reason may be, that he had allowed his steam to escape from the boiler through a smaller opening than that alluded to.

its

its avidity to absorb heat from the contiguous metal would be so much the greater.

Indeed the very friction occasioned by the violent and agitated egress through the stopcock, was likely to augment the heat in the emerging vapour, and so much the more as the pressure of the steam was higher.

I cannot help thinking that many experiments connected with heat are affected by friction, and by violent motion of the fluids employed; though I am not aware that any allowance has ever been made, or perhaps can with certainty be made, for this source of error*.

If some of these remarks should appear to be overstretched, yet the amount of the whole surely goes far to corroborate the opinion of Mr. Watt, that the latent heat of steam decreases rapidly as the temperature rises. M. Clement's experiments show, so far as they go, that the decrease in latent heat is at least as great as the increase of temperature; and that, neglecting friction, &c., the expenditure of heat in an engine of a given power, cannot be greater than in the inverse ratio of the temperature increased by 448°. But this œconomy of heat, as M. Poisson observes, does not sufficiently account for the advantage gained by high-pressure engines,—a strong presumption in favour of Mr. Watt's opinion; especially considering the many inconveniences under which high-pressure engines work.

Contrary to what we have just seen, many practical men reckon the expense of heat as the power, whatever be the pressure of the engine. But they also reckon the quantity of water converted into steam to be always proportional to the power of the machine; whereas it is certain, that the density

* The source of heat accompanying friction is still involved in obscurity. Such as take heat for a species of motion get more easily over friction than any thing else. But is it improbable, that this heat may be nearly allied to electricity, or like it suddenly collected from some distance? Supposing the absolute heat in bodies to be very great compared with what would carry their temperatures through the whole range of observation, may not much of the heat of friction, as the Marquis de Laplace alleges, be expressed from the surfaces of bodies by their mutual reaction and compression? But this hypothesis seems best adapted to solids, and would besides require a zero of temperature much more remote than where the Marquis places it, at −448° F. This zero, derived from the assumption that gases contract uniformly to nothing as their temperatures approach it, militates against the more probable opinion that all the gases admit of liquefaction and solidification. No wonder that many of the other absolute zeros are so ridiculously absurd, when they are computed on the gratuitous assumption that the specific heats of bodies are exactly proportional to their absolute heats. Many other chemical calculations are founded on the same slippery ground.

of saturated steam increases in a slower ratio than its elastic force. The experiments of Mr. Dalton and M. Gay-Lussac have completely settled this point.

But though the vaporization of water *in vacuo* require less heat as the temperature is higher, I do not suppose this to hold when the process goes on under the pressure of another elastic fluid, as for instance, the atmosphere. For I presume, an ounce of moisture dissolved in the air will, under the same barometric pressure, possess nearly the same latent heat, however much it may be diffused through the atmosphere, or whatever be the temperature; and that this latent heat will increase while the barometric pressure diminishes, and *vice versâ*. From analogy, it is not improbable it will yet turn out that the specific heat of aqueous vapour is to that of air in which it is mixed, as 8 to 5.

When an elastic fluid is compressed without liquefaction, its capacity for heat diminishes, but increases by dilatation; and it has never been shown in what degree this affects the latent or specific heats, though it is more likely the change takes place chiefly in the former. Every gas has no doubt its latent heat, as well as vapours.

Throughout their whole range, the experiments of Messrs. Dalton, Ure, and Taylor* show, that whilst the temperature increases uniformly, both the force and density of saturated steam increase *slower*† than in geometrical progression; but if the experiment of M. Clement be correct, as mentioned by M. Poisson, *Annales de Chimie*, tom. xxiii., the reverse takes place at high temperatures. Thus he found the force equal 35 atmospheres at 215° Centigrade, or 419° Fahr.; whereas, had it followed the same rate of increase as we should have been led to expect from the former experimenters, the force would only have been about half that quantity. I suppose the temperature is by an air-thermometer; but still there is something unexpected in the result. The density of such steam will be only 26·64 times as great as at 212°, and not 35 times, as many erroneously suppose.

* See Phil. Mag. vol. lx. p. 452.

† In the Edinb. Encycl. article "*Meteorology*," it is inadvertently stated, that this increase is *faster* than in geometrical progression. On the slightest reflection, however, it is obvious that the illustration there adduced from Mr. Dalton, just proves what I have stated above.

The discordance in the results of different experimenters at high temperatures is very likely owing in a great measure to differences in their thermometers. The vapour of mercury may also have operated more in some cases than in others. It seems owing to such vapours that barometers in which no air can be detected are depressed by heat, though a correction be usually applied quite in the contrary way, and of course to magnify the error.

It

It is not a little remarkable, that several respectable writers, whilst treating on the force of vapours, have been careful to point out the great œconomy of fuel which would attend the substitution of alcoholic vapour for steam as a first mover of machinery; and their only obstacle or objection to its universal adoption is its high price, though even that they suppose might in certain situations be more than compensated by the saving of costly fuel*. Their chief reason for giving such a decided preference to alcoholic vapour is, no doubt, its low temperature and small latent heat compared to those of steam having the same tension; and from which they allege an equal force could be produced in the cylinder of an engine with a much less expense of heat. But with every deference to such authorities, I cannot help thinking that the supposed œconomy of fuel from the causes just mentioned, is fully counterbalanced by other circumstances: so that upon the whole, an equal force of steam can really be produced with as little heat as the other, and probably even less, if Dr. Ure's experiments, from which I shall now calculate, be correct.

Let us compare the quantities of heat which would be expended in heating water and alcohol from 45° Fahr. to their respective boiling points of 212° and 175°, and in vaporizing them so as to produce elastic forces equal to that of the atmosphere, in two equal cylinders of engines.

According to Dr. Ure, 200 grains of such steam being condensed in 32,340 grains of water, raised its temperature 6°·5, or from 42° to 49°·5 Fahr. Hence $\frac{32340}{200} \times 6{\cdot}5 = 1051°{\cdot}05$ is the range through which 32,340 grains of steam would have heated the water, supposing the rate uniform: but to this we should add 4°·5 for heating the 200 grains from 45°, the temperature of the air, to 49°·5; and we have 1055°·55 for the heat spent in elevating the temperature of the water from 45° to 212°, and in converting it into vapour: exclusive of a compensation to be noticed presently.

Again, the condensation of 200 grains of alcoholic vapour raised the temperature of 32,340 grains of water 3°, or from 42° to 45°; wherefore, $\frac{32340}{200} \times 3 = 485°{\cdot}1$ is the rise of temperature which alcoholic vapour would have produced on its

* I am not certain whether we owe this ingenious delusion to the distinguished Spanish engineer, M. Betancourt. But it has been favourably noticed by our own countrymen, and Dr. Ure in particular has eulogized it in the most glowing terms, without seeming to have been in the least aware that it ran directly in the face of those very experiments of his own, from which he was attempting to calculate its œconomical qualities.—Phil. Trans. for 1818. Phil. Mag. vol. liii.

 weight

weight of water. But the quantities of heat thus expended in filling the two equal cylinders, will be as the above numbers multiplied by the respective densities of the vapours. Now the specific gravity of such alcoholic vapour, according to the Doctor, is 2·3 times that of steam. Hence the expense of heat in the two engines will be as 1055·6° to 1115·7°, or 60° in favour of steam.

A compensation is wanting in both cases for the heat spent in warming the glass of the condensing globe, and of the exterior vessel in the apparatus. The steam warmed it 6°·5, and the other 3°. But these compensations, if of uncertain magnitude, are proportional to the ranges through which the glass was heated, multiplied by the densities of the respective vapours; that is, in the case before us, as 6·5 to $3 \times 2{\cdot}3 = 6{\cdot}9$, which are numbers so nearly equal that the omission of corrections in that ratio cannot materially affect the comparative result; at least it gives steam no advantage over the other.

I know of no direct experiments from which I could make a comparison between the expenditures of heat in engines of higher pressure. But from Dr. Ure's experiments it appears, that when the forces of these two vapours are increased and equal, the ratio of the density of alcoholic vapour to that of steam will rather be increased; and assuming that their latent heats, though variable, maintain their former ratio*, and that the same thing holds of the specific heats of their liquids, it were easy to show that for higher pressures, the comparison would be still more favourable for steam. Similar results would follow, calculating on M. Clement's hypothesis of latent heat.

It thus appears, that the vast powers of alcoholic vapour are not so easily produced as some are apt to imagine. Nor is this the only imperfection in the scheme; for it is worthy of remark, that one gallon of water will go almost as far as three of alcohol. Add to all this, that the very grossness and consequent inertia of alcoholic vapour at high temperatures, would operate as a dead weight on a nice machine; because considerable force would be requisite to move it with sufficient rapidity through the pipes, valves, &c. The power unavoidably lost in this way, even in a steam-engine, is perhaps greater than many are aware of.

H. Meikle.

* For corrections in Dr. Ure's latent heats, see the second edition of his valuable Chemical Dictionary; and Philosophical Magazine, October 1822, and October 1825.

VI. *On the Diving-Bell.* *By* A Correspondent.

THE following remarks were written after a conversation with one of the first scientific characters of the present day. That gentleman is so unreserved among his friends, that I could not readily distinguish my own ideas from his. He has, however, published a masterly article on the subject in the *Encyclopædia Metropolitana*, and is entitled to any observations in which we coincide, perhaps, to more, if worth claiming.

There are two principal defects in the construction of the diving-bell; viz. the want of independent locomotion, and the impurity of the air:

I. It is no doubt possible to invent a submarine boat, independent of assistance from the surface; but the common bell admits an improvement of this nature. Let us suppose the frame of a carriage without the body, of sufficient weight to sink in water. Let the bell consist of wood, sufficiently light when containing air to rise with the persons in it to the surface; and let the bell be attached by a rope to the carriage frame. The persons inside by winding up the rope, may draw the bell down to the frame, and can leave it again at pleasure. The frame may be so constructed as to allow the bell to reach the ground; and the frame with the bell attached, can be moved from place to place, by any of the contrivances used for putting chairs and carriages in motion from the inside. This plan is only applicable to a level shore or sea-bank; in other cases the buoyancy of the bell admits contrivances for keeping it vertical, if the frame should be overturned. An instrument might be contrived for feeling the way before it; if, for instance, two wheels were connected with the bell by a rod, the rise of the rod in the bell would show the descent of the wheels, while its torsion would show the descent of one of them.

II. The impurity of the air arises from the following causes:

1. The vapour generated:—We are told that some stores for gunpowder on the continent are coated with sheets of lead, on which it is found that the vapour settles and runs down the sides, leaving the air dry. This is easily tried, but would perhaps only succeed in a cool state of the lead and air. Some bells are grooved for the same object, but with little effect. Perhaps absorbent substances, such as are used in Professor Leslie's experiments on the air-pump, might here be useful.

2. The temperature is a great inconvenience: by allowing the hot air to escape at the top of the bell much useful air is wasted. It might be circulated until cooled, through the ad-

joining water in a pipe; or an additional bell might be used of such a form as to expose a great surface to the water, in which it should be frequently moved about. It is worthy of remark, that any substance which would absorb vapour as before suggested, would also tend to cool the air. A metallic bell by conducting heat more quickly has an advantage over wood.

3. The generation of carbonic acid is the great cause of air being wasted; air becomes unfit for respiration by a small admixture of carbonic acid. Agitation in water will be of little use towards the absorption of the acid; lime-water might be more successful.

4. The want of oxygen:—This is generally sent down [mixed with azote in the form of common air] from the surface in barrels, or by a condensing pump; and no method can be better where the bell is fixed, a constant circulation of air obviating all the former difficulties, and not occupying the time of the persons otherwise employed below. Air may, however, be drawn down from above. If for instance, a pipe from the surface were to communicate below with the clack-hole of a common pair of bellows, and if there were a valve opening outwards fixed at the nozzle, then on opening the bellows the air would rush down the pipe, and on closing them would be driven out at the nozzle into the bell. The air in the pipe will not be of the same density as the air in the bell, on which account the pipe must not be formed of leather, or any substance which will yield to the pressure of the water. Some naturalists describe insects which rise and fall in the water, and are connected with the surface by a tube of variable length. Perhaps ultimately the oxygen will be supplied by a chemical process from the surrounding water, or from the black oxide of manganese, or condensed fluid oxygen, or some other substance taken down in the machine.

I will add two remarks on the most valuable memoir above referred to.

1. When the bell rises to the surface, the aqueous vapour condenses into a cloud. Does not this partly arise from the cold which mere rarefaction of air produces, by changing the capacity for heat?

2. In the proposal for giving motion and direction to a diving-boat, would it not be better to follow nature in the formation of fishes. Nature has placed *fins* at the *sides* of the animal in most cases. Locomotion is produced by the *tail* in a manner not generally understood; it is bent *slowly*, and returned to its straight position with *great rapidity;* as the resistance varies nearly with the *square* of the velocity, the force lost

lost in bending the tail is much surpassed by the force gained in the returning stroke. The returning stroke produces a force which may be resolved into two; one lateral, and the other in the direction of the animal's motion.

DECIMUS.

VII. *Statement of a Plan for making a minute Survey of the Heavens and for the Formation and Publication of some new Celestial Charts under the Superintendence and Direction of the Royal Academy of Sciences at Berlin*.*

Advertisement.

THE Council of the Astronomical Society are happy in being able to lay before the members, a *plan* which has been suggested for a *minute survey of the heavens;*—a grand desideratum in modern astronomy;—and, in fact, one of the principal objects for which this Society was originally established, and which it has constantly laboured to promote.

The plan, here alluded to, appears to have originated with M. Bessel, who has himself observed upwards of 32,000 of the smaller stars, situated between 15° north and 15° south declination. With a view to render *the survey of this zone of thirty degrees* more perfect (so as to comprehend many other stars not yet observed by him or by any preceding astronomer), it is proposed that it should be divided into 24 equal parts; each part containing 1^h in Æ. And that every person, who is disposed to take a share in the undertaking, should devote himself to a minute examination of all the stars situated in that portion of the heavens which may be allotted to him:—1°. by *reducing* to the year 1800 all the stars hitherto observed in that district; and *laying them down on a chart* of given dimensions: —2°. by inserting also on the same chart, *from estimation by the eye*, or from actual observation with an instrument, all the remaining stars (to the 9th and 10th magnitude) that have escaped the observation of preceding astronomers.

In order to prevent any confusion in the distribution of these portions of the heavens, it has been thought proper that the whole plan should be placed under the superintendence and direction of the Royal Academy of Sciences at Berlin: and they have accordingly issued a Prospectus, giving a detail of the plan proposed. A copy of that prospectus was forwarded to the Astronomical Society: but some of the parts requiring explanation, Mr. Herschel was requested to obtain further information on those points which appeared to be ambiguous.

* The above paper has been circulated by the Astronomical Society.

In

In reply thereto, M. Encke (the Secretary to the Academy) has addressed a Letter to Mr. Herschel, which more fully and clearly developes the views of the Academy.

Translations of the Prospectus and of the Letter, above alluded to, are subjoined. And the Council of the Astronomical Society trust that, in thus giving publicity to the Plan proposed, and circulating it amongst the members, it will be needless to add any arguments in favour of a proposal, which promises, much more fairly than any other that has yet been suggested, to accomplish so important a desideratum in modern astronomy.

London, June 9, 1826.

Prospectus.

The modern celestial charts, by Flamsteed, Bode, and Harding, contain only those fixed stars whose places at the time of their publication were astronomically determined. The number of these, however, has gradually increased from 3000 (marked in Flamsteed's catalogue and the atlas founded on it) to nearly 50,000, as given in the *Histoire Céleste* and in Piazzi's catalogue; the whole of which are marked in Harding's charts. Nevertheless, these celestial charts are very far from containing all the stars visible by means of the telescope; the number of which seems to be immense, or to increase without limit with the increased power of this instrument. Indeed we can never expect to obtain charts that are absolutely perfect; and if we aim at any degree of accuracy, it can only refer to the assumed limit of the magnitude or brightness of the stars.

Before the discovery of telescopes such a limit was fixed by the power of the eye, and the charts were capable of receiving a certain degree of perfection founded upon it. Flamsteed, however, although he added many new stars, remained far behind the perfection attainable even in his time: and it was probably the immensity of the number of the stars which prevented this great astronomer and his successors from attempting to perfect their charts beyond a certain limit, and induced them to remain contented with noticing only those stars that were astronomically determined; leaving many others unnoticed, which, although of equal brilliancy, had not yet been considered.

Nevertheless, it is desirable that we should possess charts that may be perfect to a certain limit; and the more desirable the further this limit be removed. If we determine that limit by the smallest stars yet visible through one of Fraunhofer's *comet-seekers* of 34 lines aperture and a magnifying power of 10 times, (and which can be observed without difficulty by Reichenbach and Ertel's meridian circles, provided with Fraunhofer's

Fraunhofer's telescopes of 4 inches aperture, in an illuminated field,) we shall seldom or never find a deficiency in the astronomical application of the charts, and shall obtain a result, the surpassing of which would not only be extremely difficult, but would be prejudicial for obtaining a general view, owing to the excessive number of stars which it would be necessary to introduce. But *this* detail being once attained, the charts will show us at once any thing new, on comparing any part of them with the heavens, provided the magnitude of the star be not less than the limit assumed. Besides the interest naturally attached to a more correct view of the heavens generally, and the facility thereby obtained for many astronomical observations, such charts would also offer the surest means of enlarging our knowledge of the solar system, by the discovery of new planets. Nay, such a result will be highly probable, whilst without such special celestial charts they can only be found by some lucky chance.

Indeed, there have been repeated attempts towards constructing charts of this description: and although they have not been crowned with success, it will be sufficient to enumerate the causes that have impeded their execution, in order to show that they are not now insuperable. The perfection of the celestial charts to a certain limit can only be attained by first laying down on a *net work* *, or scale, those stars that have been determined by meridional observations, in order that all the rest, intended to be introduced, may be added from estimation by the eye, perhaps assisted too by some instrument. By meridional observations alone, even if repeated more than once, we cannot acquire the certainty of having *all* the stars within the assumed limit. Even the *Histoire Céleste* contains much fewer stars than are necessary as a basis for perfect charts; wherefore it was necessary to make *de novo*, a more numerous series of meridional observations. Such a one has now been made at the observatory of Königsberg, extending over a circular zone of the heavens from $-15°$ to $+15°$ declination, and containing about 32,000 stars; which, according to an experiment made in a part of the heavens most filled with stars, are quite sufficient. Besides this difficulty, now removed in a

* [This *net work* is delineated on the copper-plate engraving which accompanied the original communication, and which was sent as a pattern. It consists of 100 small squares, formed of faint lines, half an inch (Eng.) asunder; each square comprehending a degree. It is formed on the plan, and on the same scale, as Harding's Atlas; and therefore it is unnecessary to give a specimen of it here. The plate itself is given in Schumacher's *Astron. Nach.* No. 88; and it may be seen by application to the Secretary of the *Astronomical Society.—Sec.*]

zone

zone of 30° of declination, there is another, viz. *the perfecting of the charts by the eye*, which is so laborious and requires so much time, that a single individual can make but little progress in it. This may, however, be removed by the co-operation of several; and the active zeal now prevalent among astronomers allows us to indulge in the hope that many will assist in promoting so great and useful an undertaking.

It is therefore the wish of the Academy of Sciences to unite for this object the friends of astronomy; and to procure for them every possible facility. It invites all astronomers to assist in filling up the 24 sheets of a complete celestial atlas, for which the foundation has already been laid; viz.: from $-15°$ to $+15°$ of declination and the 24 hours of right ascension: laying down at the same time the following rules to be observed in the execution.

1°. The net work, or scale, to consist of squares for the degrees of declination and right ascension: each degree measuring $5\frac{3}{4}$ Parisian lines (or 0·51 English inch). It should extend from 4 minutes of time *before* the beginning of an hour, to 4 minutes of time *after* its termination; and thus contain 510 squares.

2°. In this net work are to be marked the stars observed at Palermo, Paris, and Königsberg, reduced to the beginning of the year 1800*.

3°. The largest of them should be marked after the manner of the pattern sheet attached to the present plan: those stars which are visible only through a telescope, by larger and smaller black rings; and those visible by the naked eye, with the addition of rays†.

4°. If a star has been observed but once, the same should be marked by a short faint line projecting from one side of it; if twice, or more frequently, by two such lines, one on each side of it‡. For stars visible to the naked eye, this kind of designation would lead to indistinctness, and is in fact needless, since they are all described in Piazzi's catalogue; and therefore show, by their rays, that they have already been observed.

5°. The sheets in this state must be compared with the heavens: and all the stars, within the limits proposed for the intended

* [The stars observed at Palermo are given in Piazzi's catalogue: those observed at Paris, are given in the *Histoire Céleste:* and those observed at Königsberg, are given in M. Bessel's Observations.—*Sec.*]

† [These marks are similar to those adopted by M. Harding in his charts. The exact mode of delineating the different magnitudes may be seen in the pattern sheet.—*Sec.*]

‡ [For specimens of this mode of distinguishing the different stars, see the pattern sheet, alluded to in the note in preceding page.—*Sec.*]

intended sheet, must be estimated by the eye, as correctly as possible, and be inserted therein. And it must be observed that the stars of the chart must be such as can be seen under favourable circumstances with one of Fraunhofer's comet-seekers of 34 lines aperture, and a magnifying power of 10 times.

6°. When stars stand too closely together to be separated in the drawing, their magnitude only need be delineated, and the number of them indicated by an equal number of lines underneath it, as in the pattern sheet, $19^h\ 29^m$ and $+11°\ 55'$. Where two stars are found double-stars, *i. e.* such as are not above $15''$ or $20''$ distant from each other, they should be distinguished by such distance being mentioned: ex. gr. at $19^h\ 52^m$ and $+10°\ 12'$.*

7°. The sheet thus far advanced must be frequently compared with the heavens, partly for the purpose of discovering the changes that may have occurred during the drawing, and partly also for the purpose of finally fixing the magnitudes which the observer may be disposed to give to the stars. It will perhaps not be possible to notice in the drawing the minute distinctions between the magnitudes of the smaller stars marked on the pattern sheet, such as the 9th and (9·10)th magnitude, nor will it be essentially necessary.

One may be convinced by the pattern sheet (which represents one of the most starry parts of the heavens), that it is possible to attend to all these rules †: and that the great multitude of stars, marked upon it in the manner they are represented, neither crowd the space, nor render a general review difficult. To name and describe in such charts either the constellations and their limits, or single stars, would be both useless and injurious.

The Academy have appointed a Committee, consisting of Messrs. Ideler, Oltmanns, Dirksen, Encke, and Professor Bessel of Königsberg. And whoever is disposed to undertake the execution of a sheet, should apply to any one of the members of that Committee, who will point out to him a portion not yet undertaken by others. Such a district will remain open for him during two years; and if, after that period, he cannot show to the Committee that he has made some considerable progress in it, it will be transferred to another.

As soon as any sheet is completed it must be sent to the

* See the preceding note.

† [The greatest number of stars in any one of the squares in the pattern sheet is 16, and they are all perfectly distinct, even with the distinguishing marks attached to them.—*Sec.*]

Committee; who, after having examined and approved of it, will cause it to be engraved and published, without waiting for any others. The name of the author will be engraved on it, and any observations that he may have had an opportunity of making,—such as errors of the pen, or of the press, in lists of observations,—on stars observed, but no longer existing,—on variable stars, &c. &c.—will be published in the Memoirs of the Academy.

The Academy entertain no doubt that the fact of being able to promote, without any expensive apparatus, such a great and useful undertaking, as well as the prospect of discovering new planets even during the construction of the charts, will be sufficient to excite the friends of astronomy to participate in it. Nevertheless, it has been thought proper to announce a reward of 25 Dutch ducats for the author of every chart made according to the plan.

As the Academy enjoy the privilege of free postage *within the limits of the Prussian post*, astronomers in addressing the members of the Committee or in sending in their charts, may take advantage of this circumstance.

Berlin, Nov. 1, 1825.

Letter from M. Encke, *to* J. F. W. Herschel, *Esq.*

Berlin, May 19, 1826.

I hasten to answer the letter of the 29th of April which you were so good as to send me. I set too great a value on the interest which the Astronomical Society takes in our plan, to delay for a moment giving you all the explanation that you wish for.

The principal object of the Academy is to procure a knowledge of the heavens as perfect as the present instruments will enable us to obtain. If in Flamsteed's time we might content ourselves with possessing maps of all the stars as far as the 5th and 6th magnitude, it appears that at the present period we cannot even limit them to those of the 7th and 8th magnitude, but ought to extend them, so as to include in the same sheet all the stars of the 9th magnitude. Or at least, the continual use we make of such stars renders it desirable to possess observations sufficiently correct of all the stars as far as the 9th magnitude inclusive. If we wish to observe such stars in the same manner as Lalande has done in his *Histoire Céleste*, or Bessel in his *Zones*, we could never be certain of having observed them *all*, their number being too great. It seems, then, that we should first of all endeavour to procure a knowledge of the *whole* of the above-mentioned existing stars, more detailed

tailed than that which can be obtained by an instrument fixed in the meridian. Afterwards we may propose to make on each of those stars the necessary observations, in order to assign more accurately its true place.

Such then is the object of the new astronomical maps. They are intended as a guide to future astronomers, whereby they may know, at one view, whether there exists a star that has never yet been observed. In this point of view these new maps cannot by any means render superfluous the Atlas already published by M. Harding, which contains all the necessary details to be able to distinguish exactly in what place of the heavens a comet or a new star is seen. But the different objects of these two maps require also a different arrangement. M. Harding has taken his stars from the *Histoire Céleste* and from other catalogues, and in the regions where the observations were not sufficiently numerous, he has made up the deficiency as well by his own observations as by drawings. We wish that, in the new maps, only those stars already observed should be noted, (viz. with one or two dashes,) which are found in books that are in the hands of every astronomer; and in order not to increase uselessly their number, we propose to limit those books to the following ones; 1°. The Catalogue of Bradley (Bessel *Fund. Ast.*); 2°. Piazzi's Catalogue (Palermo 1814); 3°. The *Histoire Céleste* of Lalande; 4°. Bessel's Zones. If a star shall be found in any two of these books we may be certain that it is a fixed star; if it is found in one only, it may be a planet or a moving star. It is therefore necessary that every one who wishes to take a part in this plan should also take upon himself to reduce, to the same epoch, the observations of the *Histoire Céleste* and of M. Bessel's Zones, in order to be able to decide whether a star is either the same, or has only been affected by a very remarkable proper motion. Fortunately this reduction will be found neither difficult nor long, by means of the Tables of Reduction that M. Schumacher has caused to be computed for the *Histoire Céleste**, and by the help of those tables M. Bessel has adjoined to his Zones. I have no hesitation to assert, by my own experience, that I should be able in 8 days to compute all the necessary reductions for a whole hour in Æ: and that at the utmost 15 days would be sufficient for every case.

M. Bessel's tables of reduction give the formula

$$(1825)\ \text{Æ} = t + k + k'(\delta - \mathrm{D}) \times \cdot 01$$
$$(1825)\ \text{Decl.} = \delta + d + d'(\delta - \mathrm{D}) \times \cdot 01$$

* [Sammlung von Hülfstafeln. Vol. ii.—*Sec.*]

For example, Bessel gives for the 135th zone, the first of the ninth book,

	k		k'	d		d'	
$4^h\ 0^m$	$+0''288$	63	$+0''060$	$-56''30$	4,73	$3''68$	$D=+8^\circ$
30	$+0{,}225$		$+0{,}062$	$-61{,}03$		$3{,}73$	

whence, we have for the first five stars,

$\delta - D$	t	k	$k'\dfrac{\delta-D}{100}$	Æ. for 1825.
-17	$3^h\ 54^m\ 28^s43$	$+0^s30$	-0^s01	$3^h\ 54^m\ 28^s72$
$+44$	56 6,60	$+0{,}30$	$+0{,}03$	56 6,93
$+2$	56 45,02	$+0{,}29$	0,00	56 45,31
-16	57 28,00	$+0{,}29$	$-0{,}01$	57 28,28
-9	58 44,20	$+0{,}29$	$-0{,}01$	58 44,48

δ	d	$d'\dfrac{\delta-D}{100}$	Dec. for 1825.
$+7^\circ\ 43'\ 15''2$	$-55''44$	$-0''62$	$+7^\circ\ 42'\ 19''1$
8 44 6, 3	$-55{,}70$	$+1{,}62$	8 43 12, 2
8 1 51, 9	$-55{,}79$	$+0{,}07$	8 0 56, 2
7 43 38, 2	$-55{,}91$	$-0{,}59$	7 42 42, 9
7 50 32, 0	$-56{,}10$	$-0{,}33$	7 49 35, 6

Now it is only required to subtract the precession from 1800 to 1825, which can be done by a small table with double entry, which any one may compute for himself.

For the stars in the *Histoire Céleste* M. Schumacher's tables will in the same manner enable us to reduce the observations at once to the epoch of 1800; so that it will not even be necessary in this case to compute the precession.

I hope, sir, that these reductions, which require only the addition of three numbers, will not appear to you either too long or too complicated. They comprehend at the same time all the corrections of the instrument, and of the apparent place; and a computer ever so little versed in such calculations will not find the application of it troublesome or tedious. The degree of accuracy is as great as may be attained by any other means; since nothing indeed has been neglected in it*.

It is the desire of the Academy that each astronomer should himself make these reductions, and that he should then place these observed stars on his chart; distinguishing (in the manner above mentioned) those which have been once or twice observed. This part of the work is in my opinion a great

* [For the convenience and accommodation of those pesons who are disposed to take a share in this undertaking, the *Astronomical Society* have caused *skeleton forms* to be printed, by means of which much of the trouble and risk of error, attending the reductions, will be saved. Any number of these forms may be had, by application to the Secretary before the 1st of January next, after which day the press will be broken up.—*Sec.*]

deal

deal more difficult, and requires a more scrupulous attention than the computation, where the two columns of the values of k and d follow a regular order, and the other two columns, k' and d', have never much influence on the result. Each sheet will represent two thousand observed stars at least: every one of which will have its mode of delineation prescribed according to its magnitude and the number of observations. It will not be possible to commit this operation, which cannot even be easily verified, *to any other person than the astronomer himself;* who by putting his name to the sheet will render himself responsible for the accuracy of his work. It is highly probable that many errors, both of observation and of writing, will be made amongst that immense mass of stars which are observed only once. If the astronomer himself makes both the reduction and the drawing, he will be able to find out the cause of such errors more easily than if the whole were computed and arranged by another hand. In fact the execution itself of the drawing will render the person, who undertakes it, so well acquainted with the region he describes, that it will very much facilitate to him the accomplishment of the remaining part of the work, which consists in noting down the stars (down to the 9th and 10th magnitude) not yet observed. I think that the reduction and the drawing of the stars already observed, (made in such a manner that one may be certain that each star in the heavens corresponds to its place on the chart, which can only be obtained by making a revision of the heavens,) is about half of the whole work, and that this is also the part that has the greatest influence upon the general accuracy of the whole.

These are the principal motives which have induced the Academy to propose the plan in the manner they have done in the Prospectus. The Academy could not, as a body, itself undertake so extensive a work, and thereby render itself in some measure responsible for its accuracy. These maps will form part of the Memoirs that are published by the Society. Each member will be answerable for his own portion, and the duty of the Academy can only be that of committing this work to persons who have already given proof of their being able to fulfill the task which they engage to undertake. It is on this account that you will find, in the Prospectus, that the Academy have determined that the name of each author shall be put on his map. This is the best proof, that they do not mean to render themselves responsible for the correctness of the maps, as far as the authors are concerned: but that they intend only to defray the expenses,—to encourage astronomers by prizes,—to pay attention that a perfect conformity be kept

kept up among the observers,—to ascertain that every one, who takes part in it, intends to accomplish the proposed object,—and lastly to superintend the engraving of the maps.

The Academy had conceived this project before my coming to this situation; but their arrangements appear to me so proper, that I cannot add any thing to them to insure more fully the approbation of astronomers. I hope indeed, besides the principal object, that the discovery of comets or even of some planet, and the opportunity that it will afford to many astronomers of acquiring a more complete knowledge of a portion of the heavens, will be some of the valuable results of this undertaking. On this account it has met with considerable approbation. The greatest part of the districts are ready for distribution, and the whole will probably be finished by the time assigned by the Academy for completing the work, viz. the 1st January, 1829.

I ought to apologize if I have been too prolix, and I hope you will ascribe it to the desire I have to insure also the approbation of the Astronomical Society. I am much flattered that you should have entertained the same opinion with me, as to supplying astronomers with sheets already prepared; a method which, if it could be executed, would certainly be preferable. On my first coming here, and on being made acquainted with the views of the Academy, I thought it right to propose this idea to my fellow academicians; but having made trial of the time necessary for the execution of such a plan, I have been induced to alter my opinion. M. Harding's maps, —a work whose merit is perhaps not sufficiently known,— embrace nearly the half of the stars that have been at present observed; or perhaps about one third: nevertheless they have occupied this industrious astronomer almost twenty years. Taking into the account that part of the heavens which is not comprised between $-15°$ and $+15°$, I believe that ten or twelve years would elapse before one person, or even two cooperating for the same purpose, would be able to finish both the drawing and the engraving of the maps. My present employment does not allow me to apply exclusively to it, even if I had the confidence, which certainly I have not, that every thing would succeed well. The undertaking would in such case be put off so long that perhaps we could never be certain of finishing it. By dividing this work, however, into hours, we may hope that the honour and character of each astronomer that may take a share in it, will induce him to carry his own portion to the greatest possible degree of perfection. And if the uniformity in the drawings should not be so great as if a single person had carried on the whole, yet we shall gain in

point

point of time: and likewise have the advantage of making a revision of all that part of the heavens in the course of two years; a period very little longer than that which would be required to execute a fine engraving of the maps.

In Number 93 of the *Astronomische Nachrichten* published by M. Schumacher, there is a description of a machine which M. de Steinheil has tried and found very convenient and correct for marking the precise place of an observed star. If all the astronomers would make use of it, it would produce results having a great degree of conformity amongst the whole.

I remain, dear sir,
With the greatest consideration, yours, &c. &c.
J. F. ENCKE.

VIII. *Notices respecting New Books.*

Preparing for Publication.

A PROSPECTUS has been issued of a work under the following title:—"Illustrations of Ornithology." By Sir William Jardine, Bart. F.R.S. E., F.L.S., M.W.S., &c., and Prideaux John Selby, Esq., F.L.S., M.W.S., &c.: with the cooperation of J. E. Bicheno, Esq., Sec. L.S., &c.; J. G. Children, Esq., F.R.S.; Major-General T. Hardwicke, F.R.S.; T. Horsfield, M.D., F.L.S., &c.; R. Jameson, Esq. F.R.S.; Sir T. S. Raffles, F.R.S.; and N. A. Vigors, Esq., F.R.S.

General Directions for Collecting and Preserving Exotic Insects and Crustacea. With illustrative Plates. By George Samouelle, A.L.S. Author of "TheEntomologist's Useful Compendium."

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

Zoological Journal. No. VIII.

This Number completes, vol. ii. of The Zoological Journal, and contains the following articles:—On the small horny appendage to the upper mandible in very young chickens, by Mr. Yarrell.—Mollusca Caribbæana, by the Rev. L. Guilding, including an account of a new genus, *Peripatus.*—On the stirpes and genera of the *Pselaphidæ*, with descriptions of some new species, by Dr. Leach.—Catalogue of the Birds of the Farn Islands, by Mr. Selby.—Sketches in Ornithology, by Mr. Vigors, including observations on some species of the *Ramphastidæ*, and on a new species of Pteroglossus (*bitorquatus*).—Description of a new species of Terrapene (*bicolor*), by Mr. Bell.—On Insects which affect oaks and cherry-trees, by the late Professor Peck.—On a new fossil Astacus (*longimanus*), and on two new species of Cyprea (*umbilicata* and *melanostoma*), by Mr. G. B. Sowerby.—On the ocular points of the *Helicidæ*, by Mr. E. W. Brayley, jun.—Descriptions of rare or new subjects in Zoology, by Mr. Vigors.—On two new species of Coleoptera, *Cremastocheilus variolosus*, and *Priocera pusilla*, by Mr. Kirby.—Additions and corrections to Mr. Vigors's Sketches in Ornithology.—Analytical Notices of Books.—Zoological proceedings of Socities.

cieties.—Scientific Notices,—and Index to the volume. The Number contains six plates; and with it is published Part ii. of the Supplementary Plates to the work, containing eight engravings.

IX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 25.—THE reading of the paper On the burrowing and boring Marine Animals by Edward Osler, Esq. was concluded.

June 1.—The following papers were read: Account of some experiments relative to the passage of radiant heat through glass screens; by the Rev. Baden Powell, F.R.S.

An Account of a telescope having only one reflector, and of easy management in observing; by the Rev. Abram Robertson, D.D. F.R.S.

Account of some Experiments on the laws of electrical accumulations on coated surfaces; On the construction and use of a magnetic balance; On the electrical conducting power of various metallic substances;—all by W. S. Harris, Esq.; communicated by the President.

June 8.—The Bakerian Lecture was read, On the relations of electrical and chemical changes; by Sir Humphry Davy, Bart., Pres. R.S.

On the discordances between the sun's observed and computed right ascensions as determined at the Blackman-street Observatory; by James South, Esq., F.R.S.

June 15.—The following papers were read or announced at this meeting: Observations on a case of restoration of vision; by J. Wardrop, Esq.; communicated by the President.

On the existence of a limit to vaporization; by M. Faraday, Esq., F.R.S.

On electric and magnetic rotations; by Charles Babbage, Esq., M.A. F.R.S.

On the compressibility of water; by Jacob Perkins, Esq.; communicated by W. H. Wollaston, M.D., F.R.S.

On the figure of the earth; by G. B. Airy, Esq.; communicated by the President.

Observations for determining the amount of atmospheric refraction at Port Bowen; by Capt. W. E. Parry, F.R.S. Lieut. H. Forster, F.R.S. and Lieut. Ross.

On the crystallization of uric acid; by Sir Everard Home, Bart. V.P. R.S.

Microscopical observations on the muscular fibre of the elephant; by Herbert Mayo, Esq.; in a letter to Sir E. Home.

The Society then adjourned over the long vacation, to meet again on Thursday, November 16.

GEO-

GEOLOGICAL SOCIETY.

May 19.—A paper entitled " Notes on the Geological Position of some of the Rocks of the N.E. of Ireland ;" by Lieut. Portlock, R.E. F.G.S. was read.

In this paper the author alludes to the communications on the same subject by Dr. Berger, Dr. Buckland, and the Rev. W. Conybeare, published in the Geological Transactions.—And after some remarks on the granite and mica-slate rocks of the Mourne Mountains, the Carlingford, and another group occupying a large portion of the north of Derry (the barometrical measurement of which is given), he proceeds to notice the phænomena of the basaltic range, and to observe the connexion of the indurated chalk with the basalt; beginning at the south near Belfast, where it is underlying and almost in contact with the basalt of Mount Divis, tracing it at various points northwards to Ben Evanagh, and high up in Benbradda, and describing the gypsiferous marl, having the same dip (30° N.W.) and line of direction as the chalk, next to which and between it and the basalt, there is generally a thin stratum of ochre. To the south of the line of chalk and resting on the Dromore porphyry, a highly indurated argillaceo-siliceous schist is found, passing by various shades into a clay-stone porphyry, being, however, in its simple state, harder than the basis of the porphyry.

The author concludes, by giving his opinion that the density and crystallized structure of basalt, is not affected by the amount of pressure; and stating that he has not been able to make out any decided proof of the stratification of that rock.

June 2.—A paper entitled " On the fresh-water strata of Hordwell, Beacon and Barton Cliffs, Hants ;" by C. Lyell, Esq. F.G.S. was read.

The author, after confirming Mr. Webster's discovery of a distinct fresh-water formation on the Hampshire coast, corresponding with the lower fresh-water formation in the Isle of Wight, states that in consequence of the suspicions entertained of the possible occurrence of the upper marine formation in some of the upper strata of Hordwell Cliffs, he has examined the beds; a minute detail of which, in their order of superposition, together with the organic remains peculiar to each, is given. Bituminized wood, seeds and capsules of plants (among them *Carpolithes thalictroides* Brongn.) with fresh-water shells abound therein: and, in a bed of calcareous marl sometimes slightly indurated, from 6 to 8 inches thick, and consisting of an aggregate of *Planorbes* and *Lymnææ*, an abundance of gyrogonites (*Chara medicaginula*) was found.

 In

In the bed immediately above were discovered the scale of a tortoise, and the teeth of a Saurian, probably a crocodile.—From the presence of two species of *Serpula*, the author supposes that this series of strata might have been formed in an estuary. The shells, from the occurrence of which the existence of marine strata in Hordwell Cliff had been before inferred, prove to be species of *Potamides*, a fresh-water genus, and the beds which lie above these are exclusively fresh-water.

Of the new organic remains the valves of a *Cypris* smaller than that found in the Weald clay, but in as great profusion, are characterized as the most interesting, and a small *Ancylus* is also noted; while the presence of gyrogonites and *Carpolithes thalictroides* is quoted as completing the resemblance of the Hordwell strata to those of the Paris basin.

The author further observes, that the fresh-water strata do not crop out in Beacon Cliff, as had been supposed, but are continued for about a quarter of a mile or more in Barton Cliff, interposed between the diluvium and white sand that covers the London clay: and, scarcely hesitating to refer the white siliceous sand (which rises in Beacon Cliff and is continued through Barton as far as the high cliff near Muddiford) and, consequently, the analogous bed resting on the London clay in Alum Bay, to the fresh-water series, concludes, from the inclination of the stratum in the latter place, that the fresh-water formations suffered, though in a less degree, the disturbance to which the vertical strata of the Isle of Wight were subjected.

June 16.—A paper entitled, "Notes on the geological structure of Cader Idris;" by Arthur Aikin, Esq. F.G.S. was read.

The author, after describing the outline of this mountain-ridge, details the relative altitude and position of the different heights, the situation of the summit overlooking the crater (in the bottom of which lies "the Goat's Pool") and the various faces and slopes of the mountain.

Mynydd pen y Coed, the highest hill which stands out on the southern slope, is found to consist of beds of blueish-gray slate very regular, and rising N.E. by N. at an angle of about 35°, but bending up sharply at the N.E. end, so as to increase the angle to about 50°. The successive subjacent beds which occupy the ground to the edge of the crater, are found to consist of grauwacké; compact splintery quartz with crystals of pyrites, and in parts, ochry and cellular, and quartz-rock differing from the preceding only by being more vitreous; which last rests on a blueish-gray quartz rendered porphyritic by a few crystals of felspar. These beds all rise N.E. by N. but their

angle

angle of elevation is continually increasing, and the last forms the summit of Craig y Cae.

From hence to the margin of the crater, the space is occupied by alternations in nearly vertical beds of soft glossy slate; of coarser slate, with ochry spots and small cell; of grauwacké; of porphyritic quartz and slaty pot-stone. About the middle of the series is a single bed of brownish-gray rock, appearing to be ferruginous quartz, intimately mixed with carbonate of lime.

The next bed forming part of the summit of Cader Idris, composed of globular concretions, very hard, containing specks of pyrites, and melting in very thin shivers into a black glass, is supposed to be a trap rock.

After minutely detailing the other beds of Cader Idris, their position and angles, the author proceeds to a mountain (forming the northern boundary of the little valley wherein the Goat's Pool and another small lake are situated) extending for about two miles parallel with Cader Idris. This he calls "the stony mountain." It is composed of rounded tubercular crags and hemispherical bosses of trap, like enormous ovens rising group above group. Their surfaces are comparatively smooth, and generally reticulated with veins of quartz (which sometimes occurs in areas four or five yards across) several inches thick, of an obscurely slaty structure and adhering to the surface of the trap. Many of the groups when seen in profile appear to be of a very irregular and thick slaty structure; but when visited in front or looking down upon them, they are evidently clusters of columns, laterally aggregated, and intersected by oblique irregular joints.

The large quarry of sienite on the Tawyn Road is noticed, as showing the connexion of the trap and of the stratified rocks; and this is also shown, in a very interesting manner, on the descent northwards from Guy Graig, the eastern extremity of Cader Idris.

From these and other facts detailed in his paper, the author considers it evident that Cader Idris, and the ground between that mountain and the Mawddoch, as well as the northern boundary of the valley, consists of various well-known transition rocks, rising in general N. by E. or W.; that the beds both at the northern and southern extremities are at low angles, not greater than 20°: that the intermediate beds are at high angles, approaching to vertical; that they rest upon and are interrupted by trap rocks more or less columnar; that the trap rocks are surrounded in many places by mantle-form strata, which in some instances are obviously of the same materials as the trap, and differ only in structure; but which,

 sometimes,

sometimes, bear a less obvious resemblance to the trap, and from exhibiting transitions from that to the rocks that compose the regular strata, are probably the latter more or less changed by contiguity with the trap.

MEDICO-BOTANICAL SOCIETY OF LONDON.

April 14.—Sir James M'Grigor delivered an Address to the members of the Society on being elected President.—A communication was read on the different species of hellebore used in medicine, and on its use in maniacal cases.

May 12.—A paper entitled " Remarks on the bitter principle existing in the fruit of *Laurus persea*, and its use as a tonic medicine by the natives of Demerara;" by J. Frost, Esq. F.S.A. and L.S. Director, was read.

June 9th.—A collection of specimens of the plants enumerated in the Pharmacopœia list, was presented by William Anderson, Esq. F.L.S.—Mr. Frost delivered a lecture on the properties of *Aconitum Napallus* and *Conium maculatum*, and their narcotic principles.

July 14.—This being the last meeting of the Society during the present session, was very numerously attended; and after the ordinary business had been gone through, a paper entitled " A catalogue of the plants indigenous to Switzerland;" by John P. Yosy, Esq. was read.

Notice was given from the Chair, that communications for the gold and silver medals should be sent in before the 1st of December.

The Society then adjourned to the 13th of October.

ROYAL INSTITUTION OF GREAT BRITAIN.

May 26.—Dr. Harwood read the second part of his paper containing an account of the African Elephant, and remarks on the structure and senses of elephants in general. These were illustrated by an abundant series of specimens, many of them from the valuable collection of Mr. Brookes, and by numerous drawings.

Several accurate models of ancient buildings, in which both form and aspect had been perfectly imitated by Mr. West, were placed in the library for the evening, and also the model of a conservatory, &c.

June 2.—Mr. Solly completed his observations on the porphyry of Christiania, and gave a description of the specimens collected by himself and Professor Esmarck, in illustration of his particular views.

A peculiarly light rifle, constructed under the direction of Mr. J. H. Leigh, was laid upon the table.

June 9.—Mr. Faraday gave a history of the tunnel now constructing under the Thames at Rotherhithe, by Mr. Brunel. The progress of the undertaking from its commencement to the preceding day was detailed; the measurements and weights stated; the peculiar contrivances and arrangements made by Mr. Brunel to meet the difficulties and facilitate the progress explained; and their success illustrated by an account of various circumstances which had occurred in the progress of the work. This complicated subject was illustrated by numerous fine drawings, by a model, and by some of the actual apparatus furnished through the kindness of Mr. Brunel.*

NOTICE OF SCIENTIFIC SOCIETIES IN THE UNITED STATES OF AMERICA.†

The following enumeration of Scientific Societies in the United States was originally drawn up, at the request of a foreign correspondent, who was desirous of information respecting the progress of the natural sciences in this country. It must be considered of course as very imperfect, but it will nevertheless be sufficient to show that no inconsiderable share of our attention has been devoted to philosophical inquiries.

Considered in a geographical order, we shall mention first,

The East India Marine Society. *Salem, Mass.* This society was founded in 1799, and incorporated in 1801. It was originally instituted for the purpose of investigating and recording facts relative to the natural and physical history of the ocean. No one can be eligible as a member, unless he shall have actually navigated the seas near the Cape of Good Hope, or Cape Horn, either as master or supercargo. A blank journal is furnished to every member when bound to sea, in which he is to enter the occurrences of the voyage, observations on the variation of the compass, bearings and distances of capes, &c. and on his return he is to deliver the same to the inspector of journals. Sixty-seven of these journals have been thus collected and preserved, and a museum of several thousand specimens in Natural History has been formed. The catalogue of this collection which was published in 1821, is drawn up with considerable ability, and we have a sufficient guarantee, as well for the present activity as the future usefulness of the society, in the fact of its being under the auspices of Nathaniel Bowditch.

2. American Academy of Arts and Sciences. *Boston, Mass.* Instituted in 1780; and under the title of "Memoirs of the Academy of Arts and Sciences" have published four volumes

* See p. 72. † From Silliman's Journal, vol. x. p. 369.

quarto.

quarto. The astronomical and mathematical papers are most numerous; and the memoirs on Natural History by Messrs. Cutler, Cleaveland, and Peck, may be consulted with advantage. The paper by Mr. Cutler entitled "An account of some of the vegetable productions naturally growing in this part of the country botanically arranged," is still occasionally referred to by botanists.

3. Linnæan Society of New England. *Boston, Mass.* Instituted ——. I am not aware that this society has published any thing beside "A report of a committee relative to a large marine animal, supposed to be a serpent, seen near Cape Ann, Mass."

4. Franklin Society. } *Providence, R. I.*
5. Philophusian Society. }

The first of these societies is in active operation. A neat laboratory has been established, and the members are devoting much of their attention to the analysis of minerals. Perhaps no part of the Union offers a richer field for researches of this kind than the state of Rhode Island. As the objects of both these societies are precisely similar, we should imagine that more would be effected by a united effort, than by divided and rival institutions.

6. Connecticut Academy of Arts and Sciences. *New Haven, Conn.* Incorporated 1799. The first volume of their Memoirs was published in 1810, and contains papers by Dwight, on the *Meloe vesicatoria;* by Messrs. Silliman and Kingsley, on meteoric stones. The last part of their Transactions appeared in 1813, since which the society have apparently relaxed in their exertions. It may be mentioned that the celebrated "Experiments on the fusion of various refractory bodies," by Prof. Silliman, appeared in these Transactions. These experiments were strangely overlooked, and the priority claimed by Dr. Clarke of England, in a work published in 1820, although he could not have been ignorant that these experiments had been performed by Prof. Silliman, in conjunction with Dr. Hare, of Philadelphia, nearly twenty years previous.

7. American Geological Socity. *New Haven, Conn.* Incorporated 1819. Meet annually in September, and its meetings are held provisionally at New Haven. No separate Transactions have as yet made their appearance, but many of the communications made to the society have been published in this Journal.

8. Pittsfield Lyceum. *Pittsfield, Mass.* Instituted 1823.

9. Society of Arts. *Albany, New York.* Instituted ——; and have, under different titles, published four octavo volumes of

of their Transactions. Some interesting botanical and geological papers are to be found in these volumes; it has been recently incorporated with the Albany Lyceum, and is now known as the "Albany Institute." Arrangements are making to publish a volume of their Transactions*.

10. Utica Lyceum of Natural History. *Utica, N. Y.* Incorporated 1820.

11. Chemical and Geological Society. *Delhi, N. Y.*

12. Troy Lyceum of Natural History. *Troy.* Incorporated 1819.

13. Hudson Lyceum of Natural History. Incorporated 1821.

14. Catskill Lyceum of Natural History. Incorporated 1820.

15. Newburgh Lyceum of Natural History. Incorporated 1819.

16. West Point Lyceum of Natural History. Instituted 1824.

The greater number of these associations, although they have published no separate Transactions, are spiritedly conducted; extensive and choice cabinets are formed, and a spirit of inquiry excited which cannot fail of producing valuable results. The numerous communications of the members of these societies, are usually published in some scientific journal.

17. Literary and Philosophical Society. *New York.* Incorporated in 1815. Meet monthly for the purpose of re-

* It will hardly be considered out of place, to speak here of the *Rensselaer School*, recently established by Stephen Van Rensselaer, of Albany, which bids fair to become a nursery for Naturalists. It is now in successful operation. Its object is to qualify teachers for instructing the sons and daughters of farmers and mechanics, by lectures or otherwise, in the application of experimental chemistry, philosophy, and natural history, to agriculture, domestic œconomy, the arts, and manufactures. Mr. Eaton is Professor of Chemistry and Natural Philosophy, and Lecturer on Geology, Land Surveying, &c. Dr. L. C. Beck, a gentleman already advantageously known as a botanist, is Professor of Botany, Mineralogy, and Zoology. Well cultivated farms and workshops are established in the vicinity of the school, as places of scholastic exercise for students, where the application of the sciences may be most conveniently taught. They are also exercised in giving lectures by turns on all the branches taught by the Professors and their assistants. An ample scientific library, extensive apparatus, geological and other maps, and a very complete suite of American geological specimens, are to be found in this establishment. There is likewise an extensive collection of plants, and the most necessary specimens in zoology. Of the feasibility and great excellence of the plan of this school, and the effective character of the instruction, the trustees have already had a very gratifying proof by the exhibitions of several students. We know of no institution in our country more useful in its aim, viz. the application of science to the common purposes of life. See "Constitution and By-laws of the Rensselaer School in Troy, N. Y."

ceiving communications on subjects connected with science and literature. This society has published one quarto volume of its Transactions, and has another in the press, which is expected shortly to appear.

18. Lyceum of Natural History. *New York*. Incorporated in 1818. Meet weekly. Under the direction of this society "A catalogue of the plants growing within thirty miles of the city" was drawn up and published, and the specimens deposited with the society. Its advantageous situation for correspondence with all parts of the world, seemed to invite the establishment of a cabinet of natural history. This has accordingly been attempted; and a collection, particularly rich in minerals and organic remains, has already been formed. During the past year, forty-seven papers (excluding reports of committees on new works, which were presented by their respective authors) were read before the Lyceum. During the winter months lectures are delivered in rotation, by the members, on the different branches of natural history. In 1824, the society commenced the publication of its Annals, in a cheap form, and as materials offered. This plan of publishing occasional sheets, presents decided advantages over the course pursued formerly by literary and scientific societies. The frequent periods of publication keeps up an excitement in the society, and the members are encouraged to prosecute their researches, when assured that they will speedily meet the public eye.

19. New York Branch of the Linnæan Society of Paris. Meet annually in May.

20. New York Athenæum. This is enumerated as an association, supported chiefly by the liberality of opulent merchants, for the encouragement of science and literature in general. During the last winter, lectures on chemistry, geology, botany, &c. were appointed; and the full attendance given to these lectures was a pleasing evidence of the interest taken in these sciences.

In addition to these various institutions in the city and state of New York, we may allude to the recent establishment by law, of agricultural societies in every county in the state. Although they have but an indirect bearing upon the natural sciences, yet they are mentioned in this place, as they have originated several valuable geological Essays with particular reference to the improvement of agriculture. In 1819, an act was passed by the legislature, granting 10,000 dollars annually to the different counties in the state, in proportion to their population. It was made a proviso, that an agricultural society should be formed in each county, the members of which should raise

raise by voluntary subscription, an amount equal to the sum apportioned. The funds thus raised are distributed in premiums. Within one year after the passing of the act, twenty-six of the county societies were formed and in active operation. A central board of agriculture was organized, composed of deputies from the different county societies, and charged with the general superintendence of the whole. A further sum of 1000 dollars per ann. was granted to them by the state for the purpose of distributing seeds, &c. and publishing their Transactions, of which two volumes have already appeared.

21. Literary and Philosophical Society of New Jersey. *Princeton, New Jersey.* Instituted 1825. The declared objects of this society, as set forth in the *Discourse*, recently delived at its first annual meeting by the Rev. Dr. Miller, are "the promotion of useful knowledge, and the friendly and profitable intercourse of the literary and scientific gentlemen of New Jersey."

22. American Philosophical Society. *Philadelphia.* Instituted 1769. The earliest in point of date established in North America. It is highly creditable to this city that two scientific societies should have previously existed there for many years*. The Transactions of this society consist of two series; the first comprised in five volumes, the second in two, the last of which has just appeared. The early papers of Prof. Barton, of Mr. Jefferson on the great fossil *Megalonyx*, the geological papers

* To those who are curious in the early history of philosophical inquiry in this country, the following translation from a German traveller may be interesting. "This society is indebted for its establishment to the unwearied efforts of Dr. Franklin. For more than twenty-years previous, he had established a private society composed of his particular friends. As many crept in, however, who had little pretensions to learning, but were proud of parading among learned men, the society declined. Hence, in 1769, a new association was set on foot, without including all the former members. Those who were excluded, from a spirit of revenge established an opposition society, and elected every one, and of course some few good ones. After a time, however, for the good of science, it was deemed advisable to unite the two societies, but this did not allay the spirit of party. Many unworthy persons crept in, to the great displeasure of the elder members. These unfortunate occurrences did not, however, materially impede the advancement of science. In the year 1771, appeared the first volume of the Transactions of the American Philosophical Society, in quarto, containing many papers relative to Natural History. The war has hitherto prevented the appearance of many papers that are now ready for the press. Congress, however, although still *inter arma*, and with its very existence still precarious, has cast a favourable look upon the *musæ silentes*, and has vouchsafed to give to this philosophical society, solidity and increased activity. 'Reise durch einige der mittlern und sudlichen vereinigten Nordamerikanischen Staaten.'" Von Johann. D. Schoepf. Erlangen. 1788.

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of Mr. Maclure, and the zoological communications of Messrs. Say and Lesueur, will deeply interest the American naturalist.

23. Linnæan Society. *Philadelphia.* Instituted 1807. We believe that the members of this society have ceased to assemble for some time past. It was founded by the late Professor Barton, whose discourse "On some of the principal desiderata of American Natural History" was pronounced at the opening of this institution.

24. Academy of Natural Sciences. *Philadelphia.* Incorporated in 1818. Meet weekly. This active society has already published four volumes octavo, under the title of "Journal of the Academy of Natural Sciences," and have nearly completed a fifth volume. It has the most complete and extensive library of works upon Natural History, in the United States, for which they are chiefly indebted to the liberality of Mr. Maclure, a gentleman equally celebrated for his zeal in prosecuting scientific inquiry, and his generosity towards those engaged in similar pursuits. The Journal of the Academy is absolutely indispensable to every American naturalist.

In addition to the means of acquiring scientific information, afforded by these societies in Philadelphia, the University of Pennsylvania has a Professorship of Natural History, at present filled by Mr. Thomas Say. Dr. Hare is Professor of Chemistry; Mr. W. H. Keating, of Mineralogy, applied to the arts; Dr. Barton, of Botany; and Dr. Hewson, of Comparative Anatomy. No salaries are attached to these professorships, and they are compelled to give at least ten lectures annually.

The Philadelphia Museum was incorporated a few years since, and as a corporate body were privileged to appoint Professors. Accordingly the following gentlemen have been elected, and have already given several courses of lectures. Dr. Troost, on Mineralogy and Geology; Mr. Say, Zoology; Dr. Godman, Physiology; and Dr. Harlan, Comparative Anatomy. It is gratifying to see in one city, these various efforts to promote and extend the study of the natural sciences:—may others follow this good example.

25. Academy of Science and Literature. *Baltimore, Maryland.* Instituted 1821. Arrangements are making to commence a volume of their Transactions.

26. Columbian Institute. *Washington City.* Incorporated ——. The President of the United States is, *ex officio*, the President of this society. Under its auspices a *Florula Columbiensis* has been published, and spirited efforts are now making to establish a botanic garden.

27. Western

27. Western Museum Society. *Cincinnati, Ohio.* Established in 1818. The objects of this institution are, as stated in the public Address, to form an extensive museum of, 1. Our metals and minerals, generally including petrifactions. 2. Of our indigenous animals, embracing the remains of those now extinct. 3. The relics of the unknown people who constructed the ancient works now found in our country. Agreeably to these views, an extensive cabinet has already been formed, which is rapidly increasing.

28. Literary and Philosophical Society. *Charleston, South Carolina.* Instituted ——. This society has a choice cabinet; but has hitherto, we believe, published no Transactions. The distinguished Mr. Elliot is the President.

29. Lyceum of Natural History. *New Orleans, Louisiana.* Instituted 1825. Recent information respecting this society represents it as already in a flourishing state.

The above is as complete a list as I have been enabled to make out, and perhaps many others are still omitted. For these omissions I am not responsible, as it is extremely difficult, if not impracticable, to obtain information respecting our societies in the interior. On the spot where I am now writing, it is much easier to obtain information from Petersburg or Pavia, than from Cincinnati, Pittsburg, or Natchez. I trust to your superior means of information, for supplying all deficiencies, and remain, Respectfully yours, &c.

New York, Dec. 12, 1825. S. E. D.

ROYAL ACADEMY OF SCIENCES OF PARIS.

Nov. 28.—M. Granier, mayor of Treffort, communicated some experiments on a fruit called *Savignon*, with specimens of an oil which he had extracted from it.—M. Moreau de Jonnès presented two works in manuscript, entitled, respectively, "Geography of the plants of the French West Indian islands, to which a Caribbean Flora is prefixed," and "Geographical researches on the march of the pestilential cholera morbus in India, Persia," &c.—M. Duméril made a verbal report on M. Velpeau's treatise on surgical anatomy.—M. Damoiseau made a verbal report on the observations made by M. Littrow at the Vienna observatory.—M. Ampère read a memoir on the action exerted by an electro-dynamic circuit forming a plane curve, the dimensions of which are considered as infinitely small.—M. Poisson read a memoir, entitled, Solution of a problem relative to terrestrial magnetism.

Jan. 2, 1826.—General Andréossy, in his own name and that of Admiral de Rosily, made a favourable report on M. Moreau de Jonnès's work, entitled, "Considerations on the

the operations of war in the West Indies."—MM. Legendre and Mathieu gave an account of M. Puissant's memoir On the determination of the figure of the earth by geodesic and astronomical measurements. — M. B. de Chateauneuf's memoir On the influence of vaccination on the increase of population in France and in Paris, was referred to a committee.—M. Vicat read a memoir, entitled, New facts leading to the theory of calcareous cements.

Jan. 9.—M. Pailhès presented a table of the heights of the tides, measured at the bridge of the Tournelle, during the year 1825.—M. Jomard presented a collection of plants and vegetable productions which had been sent him by the late M. de Beaufort.—M. G. St. Hilaire presented a human monster, which had been found, embalmed, among the mummies brought from Egypt by M. Passalacqua; and he read a notice on the subject.—MM. Deyeux and Du Petit Thouars gave a favourable report on M. Granier's memoir relative to the oil he has extracted from the grains of the wild cornel-tree, called *Savignon.*—M. Girard read a notice on a new canal executing in the United States, between the Erie canal and Hudson's river.—M. Dureau de la Malle, of the Academy of Inscriptions, read an extract from a work on the Censorship among the Roman citizens from the time of Servius Tullius to that of Justinian.—M. Dumas read a memoir on proto-phosphuretted hydrogen.—M. Vicat communicated a new memoir on mortars.

Jan. 16.—M. Girard communicated a note on the theory of heat, and of chemical phænomena.—MM. Gay-Lussac, Dulong, Arago, De Laplace, and Fresnel, were appointed a committee to examine the memoirs received in competition for the prize for the best paper on the constitution of vapours.

X. *Intelligence and Miscellaneous Articles.*

A SIMPLE METHOD OF GRADUATING GLASS HYDROMETERS. BY CHARLES MOORE, ESQ.

As hydrometers of glass are irregular in shape, they are usually graduated by immersing them in fluids of different specific gravities. But as a considerable number of fluids are required, and as they are liable to change by evaporation, a different method may be found useful.

In trying the specific gravities of fluids by a bottle of known capacity, we compare together the weights of equal volumes; but in using a hydrometer we compare the volumes of equal weights; as the instrument sinks until it displaces a volume of

of the fluid equal to itself in weight. Hence we derive a method of graduating a hydrometer by the help of one fluid only.

Water, being the standard, is the most convenient; and as its specific gravity is supposed to be unity, we can easily compute how much water is equal in volume to a given weight of another fluid of known specific gravity; or in other words, with what weight a hydrometer should be loaded, in order to make it sink in water at 60°, to the point where that specific gravity should be marked: the weight of such hydrometer when finished being determined.

Let the hydrometer be loaded until it would, if permitted, sink entirely in water, and place in the stem a paper scale divided into small equal parts, taking care that some one known mark corresponds with some remarkable part of the stem. Let it then be suspended from a good small balance, as in taking the specific gravity of solids, and counterpoised by weights in the opposite scale. If a vessel of water be placed under the hydrometer, and weights taken from its counterpoise, it will of course sink and displace an equal weight of water; and in this simple and easy manner we can find the proper places for any required specific gravities; which may be written on a new scale, and put into a similar position.

For example, suppose it were desirable to make a glass hydrometer for acids and saline solutions, beginning with water, and running upwards as high as the length of the stem would allow; suppose also, that the hydrometer when immersed to the upper end of the stem was found to displace x grains of water: it is plain that x grains should be the weight of the instrument when finished; then to find the place where any other specific gravity y should be marked, $y : 1 :: x : \frac{x}{y}$, then $x - \frac{x}{y}$ being added to the counterpoise, the instrument will rise. The vessel should then be lowered a little to bring the beam horizontal, and the mark cut by the water noted for specific gravity y.

In the same manner, by simply adding weights to the scale, as many other specific gravities may be found as are thought necessary; when the scale is to be withdrawn and laid flat, and the intervals measured with a pair of compasses, and transferred to a new scale, the true specific gravities written opposite their proper marks, and the scale put in its place. The instrument may be loaded a few grains heavy, and nicely adjusted in sealing. It will show true specific gravities without referring to a table, temperature being attended to.

If it is desired to adjust a hydrometer for spirits, or fluids lighter than water,—then the scale will begin at the lower part

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of the stem, and the differences of weight being taken from the counterpoise, the instrument will sink.

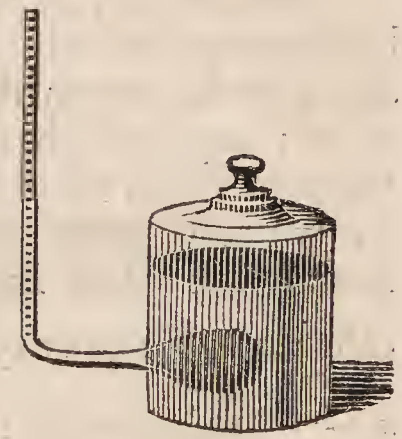
If it is wished to begin at a specific gravity different from water, suppose at 1·2, for heavy solutions, the only difference will be in loading the instrument. Thus a hydrometer that displaces 300 grains of water, must be loaded to weigh 360 grains, that it may stand at the same mark in a fluid whose specific gravity is 1·2. The specific gravity of a body is equal to its weight divided by its volume $\frac{360}{300} = 1{\cdot}2$, then to find any other specific gravity, suppose 1·25, $\frac{x}{y} = \frac{360}{1{\cdot}25} = 288$, and $300 - 288 = 12$. Twelve grains being added to the counterpoise, the instrument will displace only 288, and $\frac{360}{288} = 1{\cdot}25$. —*Dublin Phil. Journ.*

THE HYGROMETER.

Mr. Daniell's hygrometer for ascertaining the *dew-point*, or the temperature at which air deposits moisture, is the first instrument which has given precision to this branch of philosophy. It is however extremely complex, and requires *manipulation*, which is a heavy objection to any instrument. It admits a double simplification:

1st. Let the bulb of the thermometer be inserted in a dark glass bottle of æther, and let the surface of this bottle be the surface of deposition.

2dly. Instead of pouring out æther upon a bulb to cool it, let the *bottle of æther itself* cool by merely taking out the stopple, so as to allow its evaporation. The instrument will take any form, nearly as here represented.



I would suggest to meteorologists the necessity of an instrument to show the *rate of evaporation* at any given time. Mr. Daniell takes for granted, when the temperature is the same, and the dew-point the same, that the bottle of æther would always cool to the dew-point, in the *same time* measured by a stop-watch. This is by no means certain*. E. F. B.

* We have inserted this brief notice, because, at a period of science like the present, when the principles of hygrometry and of hygrometers are so much discussed, every opportunity should be allowed for the elucidation of the subject. We have strong doubts, however, of the eligibility of the modifications in Mr. Daniell's instrument, recommended by our correspondent. And with respect to his concluding suggestion, we would ask, What other circumstances are essentially concerned in the cooling to the dew-point, beside the temperature and the dew-point itself?—Edit.

DR.

DR. F. FOX'S CAPILLARY THERMOMETER.

The subjoined notice of this instrument is derived from "Notes," lately published at Derby, of Mr. D. Fox's Lectures on Anatomy and Chemistry delivered at the Mechanics' Institution in that town. We have not seen the capillary thermometer, nor have we received information respecting it from any other source.

"This instrument will detect a variation in temperature equal to the thirtieth part of a degree of Fahrenheit. The length of a degree on the scale of a common thermometer seldom exceeds one-eighth of an inch, but on this instrument the degrees are more than an inch in extent; and any person conversant with small measurements must know how visible the twentieth or thirtieth part of an inch is to the naked eye. To give an idea of the delicacy of the capillary thermometer, the lecturer stated, that eight drops of water, at the heat of 200 degrees of Fahrenheit, were dropped into a pint of cold water (at 60°) when the quicksilver instantly rose one-eighth of an inch. Here the portion of hot water was only a fifteen-thousandth part, in proportion to the cold water to which it was added. Dr. Francis Fox, in making some extremely delicate experiments upon heat, found that the most minute instruments capable of being filled in the usual way with mercury, fell very far short of the accuracy required by him. Notwithstanding, therefore, the great difficulty of filling thermometers of very fine calibres, particularly where the tube is excessively minute, as in the present instance, the Doctor succeeded in constructing the extraordinary and beautiful instrument then exhibited by the lecturer. The method pursued was this: A bulb, or ball, was blown at one end of a common thermometer-tube. This was filled in the usual way, by applying heat to the bulb, on which the atmosphere forces the mercury into the partial vacuum within the ball. This done, the tube was made red hot in the flame of the blowpipe, and drawn out into a fine capillary tube: this may be finer or larger, according to the intended delicacy of the instrument. During this operation, the capillary tube contained no mercury, which remained in the lower part of the original thermometer-tube, occupying also the whole of the bulb. A small piece of writing-paper was then tied round the extremity of the tube, so as to form a cavity, into which a little mercury is poured. The tube thus prepared is suspended by the upper end with the fingers; and the mercury in the ball being very gradually expanded by heat, in a short time rises up and fills the whole tube, until it comes in contact with the mercury in the paper cavity at the top. Then, if the instrument be allowed to cool, the mercury

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cury will pass again into the minute tube, and, by *cohesion,* will draw an additional portion after it out of the paper cavity, until the whole tube is filled at the common temperature of the air of the room. The bulb should be placed in water of the temperature to which the thermometer is desired to rise, which will cause the superfluous quicksilver to flow out at the top. The capillary tube may then be hermetically sealed and fixed on a graduated scale. The thermometer of this kind, exhibited on this occasion, was made by Dr. Francis Fox."

IODINE IN MINERAL WATERS.

Dr. Cantù has proved the existence of iodine, in the state of hydriodate, in the sulphureous mineral waters of Castelnuovo d'Asti. He infers, as a probability, from his experiments, that iodine is a constituent part of all sulphureous waters which contain muriates; and to this he attributes the medical efficacy of these waters in diseases of the glandular and lymphatic systems.—(*Giornale di Fisica.*)—*Dublin Phil. Journ.*

DISCOVERY OF RARE BRITISH PLANTS.

The *Cistus Surrejanus* has been found this year on the borders of a wood on the Addington Hills near Croydon, by Mr. W. Christy junior, who has also found abundantly, in the same locality, another plant, not generally admitted to a place in the British Flora, namely, *Dianthus barbatus.*

ATTACHMENT OF A THRUSH TO A CUCKOO.

The following anecdote of the attachment of a thrush to a cuckoo is too extraordinary to pass unnoticed.—About a month since a young cuckoo was taken from the nest of a hedge-sparrow, and a few days after a thrush, scarcely fledged, put into the same cage. The latter could feed itself, but the cuckoo its companion was obliged to be fed with a quill; in a short time, however, the thrush took upon itself the task of feeding its fellow prisoner, and continues so to do with the utmost care; bestowing on the cuckoo, which is nearly twice as large as its foster parent, every possible attention, and manifesting the greatest anxiety to satisfy its continual cravings for food. The birds are in the possession of Gideon Mantell, Esq. of Castle-place, in this town. The correctness of this statement is, therefore, unquestionable.—*Lewes Paper, June* 25.

TUNNEL AT ROTHERHITHE.

At the concluding Friday-evening meeting at the Royal Institution, on the 9th of June, an account was given of the present

present state of this Tunnel, the plan of which was described in the Phil. Mag. vol. lxxii. p. 139.

Numerous fine drawings and sections were hung up in the lecture-room, and upon the table was a model illustrative of one part of the apparatus now in use; and also some of the smaller parts of the apparatus itself. The principle and proceedings which have advanced the work to its present state were explained from the table by Mr. Faraday, for Mr. Brunel. A tower of brick-work was first erected upon an iron and wooden curb, furnished beneath with a cutting edge; this tower or cylinder was tied together by forty-eight vertical bolts, half iron and half wood, and by thirty-seven horizontal and imbedded wooden hoops. The tower was forty feet high, fifty feet external diameter, three feet thick, required 250,000 bricks, and 1000 barrels of cement, and weighed about 1000 tons. The mode of sinking this cylinder was then described, first, by removing the short piles on which it had been built, and then by taking away the earth from the inside; and the complete command of the tower during its descent explained and illustrated. Being, with the exception of seven feet, sunk into the earth, it was underpinned for twenty-four feet, and then a second smaller cylinder was lowered in the same manner, at the bottom of the first, for the purpose of a reservoir. This was described, as also the manner in which this enormous shell of brick-work was completed, and was, and is still, preserved from injury by the pressure of the surrounding earth and water; the whole mass weighs about 2000 tons, and, notwithstanding, is buoyant by about 150 tons. The depth from the top to the bottom is about eighty feet. The advantages of this process of sinking the tower consists essentially in dispensing with a coffer-dam, and the consequent diminution of expense; in the comparatively small quantity of ground required on the surface; and in the utter absence of all interference with the neighbouring houses: although surrounded by houses on all sides, within twenty-five feet, not the slightest shake or disturbance has been occasioned.

The horizontal progress was then described, and the peculiar frame-work by which Mr. Brunel makes safe progress in any kind of ground illustrated by large sectional drawings. The section of the brick-work is thirty-six feet six inches by twenty-one feet six inches; and the section of the two ways, each thirteen feet six inches wide, by sixteen feet high. The work has been carried forward 130 feet, the tunnel being completed immediately up to the frames. The numerous accidents of ground, and the manner in which they were met and obviated by the apparatus, were strikingly illustrative of its

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powers, and the forethought of the contriver; and these were further shown in the precautions ready for circumstances which have not as yet occurred. Every foot advance requires the removal of forty tons of earth, which has to be replaced by seventeen tons of brick-work, and requires 4000 bricks. It is expected, that when in full working order three feet will be done per day; work having been done up to 30 inches per day with the till now incomplete arrangements; and as much as 100 tons of earth per day having been sent up for a week together.

LIST OF NEW PATENTS.

To Daniel Dunn, of King's Row, Pentonville, for improvements on the screw-press used in the pressing of paper or tobacco, or in the expressing of oil, extracts, or tinctures, and for various other purposes.—Dated the 23d of May, 1826.—6 months allowed to enrol specification.

To Thomas Hughes, of Newbury, miller, for his improvements in the method of restoring foul or smutty wheat and rendering the same fit for use.—23d of May.—6 months.

To Francis Molineux, of Stoke St. Mary, Somersetshire, gentleman, for an improvement in machinery for spinning and twisting silk and wool, and for roving, spinning and twisting flax, cotton, &c.—23d of May.—6 months.

To Thomas Parrant Birt, of the Strand, coach-maker, for his improvements on wheel-carriages.—23d of May.—2 mon.

To John Parker, of Knightsbridge, Middlesex, iron and wire-fence manufacturer, for improvements on, or additions to, park or other gates.—23d of May.—6 months.

To Dominique Pierre Deurbroucq, of Leicester-square, for an apparatus to cool wort, or must, previous to fermentation, and also for the purpose of condensing the steam arising from stills during the process of distillation.—23d of May.—6 mon.

To William Henry Gibbs, of Castle-court, Lawrence-lane, London, warehouseman, and Abraham Dixon, of Huddersfield, manufacturer, for a new kind of piece goods formed by a combination of threads of two or more colours,—the manner of combining and displaying such colours in such piece goods constituting the novelty thereof.—23d of May.—2 months.

To Joseph Smith, of Tiverton, Devonshire, lace-manufacturer, for an improvement on the stocking-frame and improved method of making stockings, &c.—23d of May.—6 months.

To John Loach, of Birmingham, brass-founder, for a self-acting sash-fastener, which fastening is applicable to other purposes.—23d of May.—6 months.

To Richard Slagg, of Kilnhurst Forge, near Doncaster, steel manufacturer, for an improvement in the manufacture of

of springs chiefly applicable to carriages.—23d of May.—6 months.

To Louis Joseph Marie Marquis de Combis, a native of France, but now residing in Leicester-square, for an invention of certain improvements, communicated from abroad, in the construction of rotatory steam-engines and apparatus connected therewith.—23d of May.—6 months.

To James Barlow Fernandez, of Norfolk-street, Strand, for improvements in the construction of blinds or shades for windows or other purposes.—26th of May.—6 months.

To Robert Mickleham, of Furnival's Inn, London, civil-engineer and architect, for improvements in engines moved by the pressure, elasticity, or expansion of steam, gas, or air; by which a great saving in fuel will be effected.—6th of June.—2 months.

To Henry Richardson Fanshaw, of Addle-street, London, silk-embosser, for an improved winding-machine.—13th of June.—6 months.

Obituary.—John Templeton, Esq. A.L.S. &c.

As neither the decease, nor the scientific labours, we believe, of this gentleman, have yet been recorded in any permanent medium of scientific information, though some months have elapsed since his death, we think our readers will not be displeased with the following neat obituary notice of him, extracted from "The Irishman" Belfast newspaper of December 23, 1825.

"On Thursday, the 15th December, 1825, died John Templeton, Esq., of Cranmore, Malone, near Belfast, aged 60 years —a man of primitive simplicity of character, and eminent scientific acquirements. The early part of his life was not much distinguished from that of country gentlemen in general, being addicted to shooting, and other rural occupations; but for more than thirty years past, his attention had been actively and successfully turned to the study of botany, natural history, geology, and mineralogy. In the first of these branches, and especially in the more abstruse and difficult departments, he attained a rank equal to any botanist in these islands, and probably to any in Europe. He was an able and expert draughtsman, and possessed a singular facility in taking accurate likenesses of vegetables and animals; and by a diligent use of this enviable talent, he has left an immense collection of drawings, many of them of the rarer plants of the country; and those for elucidating the natural history of Ireland are of unequalled extent and value. The modesty of his nature, and a wish to bring his scientific researches to still further perfection, made

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him withstand all solicitations to give the fruit of his labours to the public; and on this account, many less scrupulous, and with inferior pretensions, obtained fame and emolument, which should more justly have been the recompense of his talents.

"As a man, the late Mr. Templeton was highly esteemed by a large circle of acquaintances: his downright honesty and scrupulous fidelity, in all the relations and transactions of life, were universally appretiated; and in Ireland's most evil days he still held fast his intergrity. He was one of the very first promoters of the Belfast Academical Institution, and to the last hour of his life a zealous friend to its prosperity and independence. For the poor, his time, his services, and his property, were ever ready; and an abhorrence of cruelty and oppression pointed him out as the natural resource of the unfortunate in his neighbourhood, for whom, however, his exertions were often frustrated by the apathy of others, and the want of necessary cooperation.

"He was a strenuous and enlightened advocate of civil and religious liberty—conceiving that rational beings in society should be governed by reason; and that the ruling powers of a nation who did not make the unfolding of the human faculties their principal object, and the improvement of the moral character the rule of their conduct, were not entitled to the name of a government. He conceived religion to be not a matter of human regulation, but strictly a personal affair betwixt every man's conscience and his God—to his own master he must stand or fall;—and he abhorred the gross profaneness which would pervert it into a test for civil rights, and the gross hypocrisy which would make it a cloak for the acquirement of emolument or honour.

"His opinions further on this subject are probably known to few; and those from whom they differ may cast over them that veil of charity which he was so ready to extend to the conscientious opinions of others. The plainness of his manners bespoke the sincerity of his heart, which expanded with benevolence for the whole human race, but kindled with extraordinary warmth in the circle of his family and friends.—To them, his loss is irreparable; but it is hoped that his various scientific collections and drawings will yet be forthcoming as a valuable bequest to the public, equally beneficial to his country and honourable to his own name and family.

"The members of the Natural History Society of this town, with a feeling most creditable to themselves, assembled by appointment at his funeral, and, as a peculiar remark of their attachment and respect for his talents and character, bore his remains from the hearse to the grave."

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The writer of the above, we may observe, does not appear to have been aware, that an interesting paper by Mr. Templeton, on the naturalization of plants, was published in the eighth volume of the Transactions of the Royal Irish Academy, and reprinted in Nicholson's Philosophical Journal for July 1803.

Results of a Meteorological Journal for June 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.

This month has been remarkably dry, warm, and calm, with prevailing winds from the N., N.E., and N.W., and hot sunshine at intervals.

No measurable rain fell here, except on the 1st, 7th, 27th, and 30th; and the sun having attained and is now returning from his greatest North declination, the heat at the earth's surface, after an unprecedentedly dry spring, is powerful. But it is not only the descending caloric contained in the great angle which the meridional solar rays make with the visible horizon at this time that causes us to feel the heat so oppressive, but the combined effect of the incident and reflected rays, and radiations, by which we are placed as it were between two fires, or in a double heat; and so long as the rain keeps off in a peculiarly dry season, the heat naturally increases, because the nocturnal dews, however copious they may be in this latitude, are not sufficiently heavy to lay the dust, and lessen the heat retained in its transparent particles. It is remarkable that we have not had so dry a spring, or so small a quantity of rain as the last afforded, since 1806,—a period of 20 years.

The 27th was a very hot sultry day and night; as soon after noon the thermometer in the shade, in a northern aspect, rose to 86 degrees, and a dead calm was observed at intervals, with glows of descending heat from a bed of *cirrocumulus* in the zenith, which confined the heat downwards, and raised the thermometer as high as its *maximum* for the last summer. In the early part of the morning there was a heavy thunder-storm at Southampton and in other parts of Hampshire; only a light shower of rain fell here, but the storm was distinctly traced by the blackness of the sky. At 2 o'clock P.M. a perfect anthelion appeared for about two minutes, on the right side of a large *cumulostratus* cloud, which had the striking appearance of an unornamented crown and cushion: the anthelion resembled the sun's disc divested of its rays, as we sometimes see it by the intervention of a light attenuated cloud; it was about 95 degrees distant from the true sun, with an altitude of

of nearly 40 degrees, and a point and a half to the eastward of due North.

In the afternoon of the 28th it was very sultry in London, when the heat and sudden inosculation of the clouds brought on a heavy thunder- and hail-storm, which did considerable damage, particularly in its neighbourhood.

Hay-making was commenced generally in this neighbourhood in the middle of the month; and the hay, which was much short of an average crop, was got in in a few hot sunny days without rain. The wheat came into ear at the beginning of the month, and will be fit for the sickle in the middle of July; and there is every appearance for average crops. Fruits of all kinds, particularly grapes, which bloomed well, have been much improved in appearance by the warm weather.

Lamentable complaints prevail in several parts of Scotland respecting the effects of the solar heat and continued drought upon the pastures and vegetation, and the great probability, without rain, of a very scanty corn harvest. The effects of the continued heat and drought, although they have been very injurious to the pasture lands, are not, however, so alarming in the South of England.

The mean temperature of the external air this month is nearly four degrees higher than the mean of June for the last ten years, and within one degree and six-tenths of the hot June in 1822. The mean temperature of spring water has increased nearly two degrees this month; and the average evaporation was one-fifth of an inch per day.

The atmospheric and meteoric phænomena that have come within our observations this month, are, one anthelion, three solar halos, two meteors, one rainbow; sheet lightning emanated from the clouds in the nights of the 26th, 27th, and 30th; and four gales of wind, namely, one from the N. and three from N.E.

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·48,	June 20th—Wind S.E.
	Minimum	29·84,	Ditto 1st—Wind N.W.

Range of the mercury . . 0·64.

	Inches.
Mean barometrical pressure for the month	30·230
——— for the lunar period ending the 6th inst. . .	30·019
——— for 18 days, with the Moon in North declin.	30·100
——— for 12 days, with the Moon in South declin.	29·938
Spaces described by the rising and falling of the mercury	2·820
Greatest variation in 24 hours	0·240
Number of changes	19·

Thermometer

Thermometer	Maximum	86°, June 27th—Wind	W.
	Minimum	50 Do. 3rd—Wind	NW.

Range 36

Mean temp. of the external air 65·28

——— for 31 days with the Sun in Gemini . . . . . 62·43

Greatest variation in 24 hours 28·00

Mean temp. of spring water at 8 o'clock A.M. . . . 50·73

De Luc's *Whalebone Hygrometer**.

	Degrees.	
Greatest humidity of the air .	47	in the evening of the 29th.
Greatest dryness of ditto . . .	28	in the aftern. of the 23d.
Range of the index	19	
Mean at 2 o'clock P.M. . . .	37·2	
—— at 8 o'clock A.M. . . .	40·3	
—— at 8 o'clock P.M. . . .	41·0	
—— of three observations each day at 8, 2, and 8 o'clock	39·5	

Evaporation for the month . . . . 6·00 inch.

Rain in the pluviameter near the ground . 0·895

Rain in ditto 23 feet high . . . . 0·845

Prevailing winds, N., N.E., and N.W.

Summary of the Weather.

A clear sky, 8; fine, with various modifications of clouds, 16½; an overcast sky without rain, 4½; rain, 1.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus
23	16	21	0	23	20	7

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
7	7	2	3½	½	2	1	7	30

Our Boston correspondent states that June has been remarkable for the small quantity of rain which has fallen. But on the 1st of July the rain fell in torrents, which in a short time raised the pluviameter to 1·44 of an inch. This rain after so long a drought has been a great relief to that town and neighbourhood. The hot weather continues:—July 3. Ther. 72°.

An immense quantity of Ladybirds (*Coccinella*) have fallen in every part of the town, and the fishermen say that at sea they were covered with them.—*Brighton Gazette.* [They have appeared also in extraordinary abundance in and about London.—*July* 28.]

* For 22 days.

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* J. CARY *in London, and Mr.* VEALL *at Boston.*

Gosport, at half-past Eight o'Clock, A.M.						Clouds.							Evaporation.	Rain near the ground.	Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.			Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
June 1	29·88	56	49·95	...	N.	...	...	...	...	...	1	1	...	0·780	29·86	29·60	51	59	54	55	...	...	Cloudy	Cloudy	NE.
2	29·88	58	......	...	N.	...	1	1	...	...	1	...	...	......	29·94	29·60	50	55	53	55	...	·06	Rain	Rain	N.
3	30·09	60	......	...	N.	1	1	1	...	1	1	...	0·40	......	30·10	29·73	56	62	51	57·5	...	...	Fair	Fine	NW.
4	30·26	61	......	...	NW.	1	...	1	...	1	1	...	...	......	30·21	29·80	59	64	58	64	...	...	Cloudy	Cloudy, Rain *p.m.*	W.
● 5	30·30	60	......	...	N.	1	...	1	...	1	1	...	...	......	30·31	29·93	55	64	57	59·5	...	·13	Fine	Fine	S.
6	30·35	61	50·00	...	NW.	1	1	1	...	1	...	...	·60	.....	30·30	29·92	56	68	60	61·5	...	...	Fair	Fine	S.
7	30·25	65	......	...	N.	1	1	1	...	...	1	1	...	·015	30·26	29·85	61	64	54	59·5	...	...	Cloudy	Cloudy	S.
8	30·24	60	......	...	NE.	1	...	1	...	...	1	1	...	......	30·24	29·93	57	65	55	62	...	...	Cloudy	Cloudy	E.
9	30·00	66	......	42	NE.	1	1	1	...	1	1	1	·65	......	30·02	29·60	64	71	56	63·5	...	...	Fair	Fine	E.
10	29·99	64	......	41	NE.	1	1	1	...	1	1	...	...	......	30·00	29·63	60	70	57	63	...	...	Fair	Fine	E.
11	30·08	66	......	41	N.	1	1	1	...	1	1	...	...	......	30·10	29·70	60	69	60	64·5	...	...	Fine	Fine Ther.	NE.
12	30·26	69	......	41	E.	1	1	1	...	1	1	...	·65	......	30·29	29·78	65	74	64	69	...	...	Fine	Fine, 2 *p.m.* 79	calm
☽ 13	30·30	69	50·50	40	NW.	1	...	1	...	1	...	...	...	......	30·25	29·74	65	77	65	73	...	...	Fine	Fine, 2 *p.m.* 83	calm
14	30·30	68	......	42	NW.	1	1	1	...	1	1	...	...	......	30·26	29·70	64	76	64	70	...	...	Fine	Fine	W.
15	30·26	65	......	41	NW.	1	1	1	...	1	1	...	·60	......	30·13	29·61	65	75	58	68	...	...	Fine	Fine	W.
16	30·29	59	......	38	N.	...	...	...	...	1	1	...	...	......	30·30	29·83	59	65	55	58·5	...	...	Fair	Fine	NW.
17	30·44	63	......	38	NW.	1	1	1	...	1	...	...	...	......	30·34	29·94	59	70	55	59	...	...	Fair	Cloudy	NW.
18	30·38	68	......	41	NW.	1	...	...	...	1	1	...	·60	......	30·30	29·80	69	74	64	70·5	...	...	Fine	Fine	NW.
○ 19	30·40	68	......	39	N.	1	...	1	...	1	1	...	...	......	30·40	29·95	65	68	54	64	...	...	Cloudy	Fine	E.
20	30·48	63	50·95	38	SE.	1	...	...	...	1	...	...	...	......	30·47	30·06	58	69	55	65	...	...	Fair	Fine	E.
21	30·42	60	......	37	NE.	1	...	...	...	1	1	...	·55	......	30·45	30·03	59	64	54	64	...	...	Cloudy	Cloudy	NE.
22	30·34	60	......	41	NE.	...	...	...	...	1	...	...	...	......	30·36	30·	61	65	54	57·5	...	...	Fair	Cloudy	N.
23	30·34	64	......	40	NE.	...	...	...	...	...	...	...	...	......	30·45	30·05	56	69	56	59	...	...	Fine	Cloudy	E.
24	30·39	67	......	40	NE.	...	...	...	...	...	...	...	·80	......	30·42	30·	60	75	60	68	...	...	Fine	Fine	E.
25	30·34	69	51·40	39	NE.	...	...	...	...	...	...	...	...	......	30·36	29·90	68	74	65	71·5	...	...	Fine	Fine 2 *p.m.* 81	SE.
26	30·23	75	......	40	E.	1	1	1	...	1	...	1	...	......	30·26	29·75	70	80	69	72	...	...	Fine	Fine	calm
☾ 27	30·04	72	......	40	NE.	1	1	1	...	1	1	1	·65	·010	30·05	29·50	73	81	71	75	...	...	do. th^r. r^n.	do. th^r. & l^g. at nt.	calm
28	30·06	76	......	41	E.	1	1	1	...	1	...	...	...	......	30·10	29·40	74	84	70	75·5	...	...	do. shw.	Fine, do. with rain.	W.
29	30·20	75	......	43	SW.	1	1	1	...	1	1	...	...	......	30·18	29·50	69	79	68	73	...	·02	Fine	Fine	W.
30	30·22	70	51·60	44	NE.	1	1	1	...	1	1	1	·50	·090	30·19	29·58	70	79	71	73	...	...	Fine	Fine	W.
Average.	30·234	65·23	50·73	40·3		23	16	21		23	20	7	6·00	0·895	30·29	29·78	61	70	60	65·	1·04	0·21			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *AUGUST* 1826.

XI. *Some Account of the Spherical and Numerical System of Nature of M.* Elias Fries. *By* John Lindley, *Esq. F.L.S., &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

My dear Sir,

AT a time when the doctrines of spherical and numerical divisions in nature are attracting much attention, it cannot but be interesting to the public to be put in possession of the latest opinions of M. Fries, who is the founder of the system of quaternary arrangement, and the authority to which the most philosophical of our writers upon the subject has so repeatedly referred. These opinions are contained in the Introduction to a work published by M. Fries in 1825, under the name of *Systema Orbis Vegetabilis*, and may be said to exhibit the most condensed and well-arranged statement of the theory which has yet appeared.

As the work is at present very rare in this country, the following abstract of such part of its contents as immediately refers to the principles of the theory may possibly deserve your notice.

I make no comment, at this time, upon the laws of this or similar systems, which are far too difficult and important to be hastily discussed. I give M. Fries's words to the public as nearly as possible, in the order in which he has arranged them, and, I hope, in all cases in the sense of the author, in order that those who are friendly to the original opinions of that very acute reasoner may have the advantage of knowing the present state of his ideas, and of seeing how far they agree or disagree with the various theories which have been lately proposed among ourselves.

Such points only as have appeared to me to apply less to the immediate principles of the system than to the application of them have been omitted.

I am, my dear Sir, yours truly,

Acton Green, July 19, 1826. John Lindley.

§ 1. Na-

§ 1. Nature is an universal complication of phænomena existing and acting in all places and at all times—an infinite power made manifest by the successive evolution of a finite power,—the sum of the whole creation in a continuous state—all existent matter proceeding from perfection and pregnant with futurity.

In nature there is a perpetual struggle, an uninterrupted rotation. The powers of formation and destruction operate alternately, whence nature is always dead and regenerate. The human mind viewing this last phænomenon in its most extensive and at the same time most satisfactory sense, calls eternity in a state of ceaseless variation by the name of NATURE.

§ 2. Nature must be considered as either perfect or approaching perfection (*vel ut naturans vel ut naturata*).

§ 3. The powers and the productions of nature are coexistent.

All power is as it were a law under which a given production holds its existence, but in such a manner that all power is the finite revelation of an infinite law. To act and to exist is the same thing.

Power therefore is nature without production; Production is matter without power. Neither exists in nature by itself.

§ 4. All the powers of nature are more or less perfect manifestations of one primitive power, which acts by its different productions according to the same eternal, immutable, absolute laws. But the powers of nature act only by mutual reaction; so that each power of nature becomes in its products impeded, interrupted, or quiescent.

The most perfect primitive power appears nowhere in nature absolute; but more or less impeded. Hence the powers of nature are various, some one among them being more perfect or active than the rest (less impeded).

The existence of nature depends upon this kind of control, and successive evolution; every power which is absolute and independent of restraint becomes infinite, and ceases to be perceptible as a finite power—(Nature).

Powers of low degree act upon those of higher degree; but the lowest powers, when not struggling with higher, contain opposing principles in themselves; for example, attraction (repulsion), electricity, magnetism, &c.

This opposition, which pervades all nature, is called *Polarity.*

The more agreement there is between powers, the greater also the agreement between their productions.

The more perfect a power, the more complex its actions; the more perfect its productions.

The more complex are actions, with the more difficulty are their laws explained: thus, for example, the laws of affinity and of motion are almost ascertained; by no means those of vitality or of sensation.

§ 5. All things which exist in nature are a whole, and at the

the same time a part of a larger whole. They are capable of being themselves resolved into other wholes until the human mind sinks under ideas of sublimity and subtilty which are imperceptible to it,—of the universe and of atoms.

An atom is a whole (an individual), a plant is a whole, the earth is a whole, the universe is a whole: hence all things which exist are parts of one highest whole.

The vital principle of every individual is one; the same vitality animates the universe; so that there is one and the same primitive power which is revealed, by divers phænomena, in divers degrees of perfection. § 4.

Let us imagine all nature to be an immense sphere; all the rays converging in the *centre* where they finally become confluent in a point, which may be called the point of *identity*. This point comprehends the perfection of all the rays; for that the most perfect and most completely formed creations, as the sun, are always situated in the centre, is testified by all authority, by all experience.

The powers of nature diverging from each centre in polar opposition, are continually passing into opposite series. A new sphere is formed by each opposition, whence the highest (most perfect) sphere is again and again resolved into new spheres which form wholes of themselves, and each of which, according as its power is a more or less perfect evolution, in itself reflects the whole in a more or less distinct degree.

The centres of these spheres may be exceedingly distant from each other, but their rays always impinge upon the rays of some other sphere: hence they are not the most perfect forms (*summa*) of each section which run into each other, but those which are least perfect (*infima*).

The different spheres therefore, being dependent upon the same eternal laws, and only varying according to the idea peculiar to each sphere, answer the one to the other. Hence among all natural productions, a *more near* or *more remote* resemblance is perceptible; the one of them being such resemblance as exists between subjects contained in the same sphere:—*Salts*, for example, which are formed of the same basis combined with different acids; the other being such resemblance as exists between subjects contained in different spheres of the same degree of evolution, as Isomorphous Salts, the bases of which are different but the form the same, on account of the identical relation of their elements. The former is called *Affinity*, the latter *Analogy*.

§ 6. It is impossible for the human mind, itself a finite creation, to regard nature, whether her powers or her productions are considered, in the light of the whole manifestation of an infinite power, but only as parts or fragments of such manifestation. But to comprehend these as one whole, that is, as an eternal and immutable yet ever varying body, or, as innumerable forms of one highest whole, is the end of all disquisition, the sum of which we call a *System*.

It is necessary not to confound with systems, properly so called, those indexes of nature which are incorrectly called *Artificial Systems.* Indexes have references only to names, systems to ideas. "Tum primùm homines res ipsas neglexerint, quum nimio studio nomina quærere inciperent."—Galen.

§ 7. A system contains within itself the seeds of some more complete evolution, but it does not admit of arbitrary alterations.

Not that any absolute system can ever be contrived; for I am by no means of the opinion of those who expect that a system is to be as unchangeable as if it were petrified.

§ 8. If nature be closely pursued, a system is called *Natural;* if this Ariadnean thread be not followed, it is called *Artificial* or factitious.

There is, however, no absolutely natural system; such is only ideal: neither is there any merely artificial system; because its principles must necessarily be borrowed from nature herself.

Besides, nature wholly disavows our sections, she being a whole; all systems, therefore, as far as their arrangement is concerned, are necessarily artificial.

It is by the comparison of various systems with each other that our notions of such as are natural and such as are artificial are acquired, those having the former designation which press most closely upon the footsteps of nature. Hence it is that a system which is today called natural, becomes tomorrow, by the accession of new ideas, artificial; as that of Tournefort, &c.

Is it not then a vain labour to search after a natural system, since such will never be found; and are not all attempts at it rash, until every thing which is capable of observation shall have been observed? If this were admitted, it would be useless to seek for perfection in any thing; for we can never hope that our experience will be perfect; and there will be no want of subjects for examination to a person who shall live a thousand ages hence. Such sublime truths as the present age shall strike out, are therefore not to be contemned because they will become more full and perfect hereafter.

§ 9. A system of nature proceeding from subjects of the most simple organization to such as are more perfect, or from the circumference to the centre, is called a Mathematical System.

For mathematicians assume that nature herself proceeded from forms of the most simple kind to those which are more perfect; and that that therefore is the most natural road, which nature herself has followed in forming her creations.

All natural bodies, indeed, originate in successive development, yet in a contiguous series within a determinate sphere. Every new sphere originates in a digression from a series which is otherwise continuous. Whenever a more perfect sphere is separated from one which preceded it, and has acquired a higher station than its parent, it may be itself pressed down by such new ones

as

as emerge from itself; but the depressed sphere is also capable of continuation in its descent, and under this mode of development the same principles and the same types are regenerated under more perfect forms in the higher spheres.

Those therefore are mistaken who assume that nature proceeded in a simple series to her most perfect productions. Thus, for example, all parasites, both animals and plants, must necessarily have been created later than their matrix (and should therefore be the most perfect parts of the creation). But Fungi, which are the latest in the series of vegetable development, are the most simple of all in their structure.

In Minerals, of which the most simple are at the same time the most perfect, the Mathematical system may be employed, because it corresponds with the Philosophical. But in higher spheres, in which vitality must be considered, the laws of mathematics are of no avail.

§ 10. A system of nature which takes for the basis of its arrangement the order of development of individuals is called Physiological.

But take care not to imagine that the first series of evolution is a simple one. As the evolution of the animal and vegetable kingdom may be said to have proceeded with nearly equal paces, so the different sections of vegetables cannot be said to have arisen out of a simple series, but out of parallel or radiant series. Many Algæ must have been created more recently than the most perfect plants, Entozoa than the most perfect animals. Whence it is to be inferred; 1st, That nature, properly speaking, can only be said to have proceeded from the most simple forms to those which are more compound, in theory (*de ideis*); but, 2dly, to have often operated in an inverse order in her forms.

§ 11. *Philosophical systems* do not depend upon individual productions which are subject to continual variation, but upon eternal and unchangeable ideas. These always proceed from the centre to the circumference, or from the most perfect productions to those of a lower order.

This is the method of my Mycological system, and it agrees with the mathematical system if the order be inverted.

A Philosophical system depends upon the laws of logic; for the laws of logic are by no means notions contrived by man, but eternal and immutable, and established by Nature herself. As the rotation of the heavenly bodies, discovered after the laws of mathematics, must necessarily follow those laws; so also no observation in nature can invalidate the laws of logic. For the laws of logic are the laws of nature.

It must be observed, however, that a system, although logically true, may be naturally false, because it may have been deduced from false principles; but every true system cannot deviate from the rules of logic.

§ 12. A Philosophical system is superior to all others.

It may at first appear, perhaps, of little moment, what way we follow

follow in enumerating the productions of nature; but if one way is more certain and more facile than another, that is surely to be preferred.

To me it appears most advisable to commence with that which is most perfect, most completely developed, and therefore most easily understood; and thence to descend to forms of a more imperfect kind, and therefore of a more doubtful nature. The half-developed portions of the lower forms would never be understood, if they were not more completely developed in the higher forms. This is the path which is pointed out both by experience and common sense; the idea of a seed is not derived from an *Uredo*, nor that of a vegetable from an *Erineum;* but the reverse.

This is especially true of those lower spheres which bring up the rear: the last point of simplicity will never be attained, and will never be determined; although our microscopes are daily extending our views, the poles of vitality will never be reached. It is better therefore to set out from a *certain* point (the centre) than from an *uncertain* point (the circumference) which may be extended to infinity.

So it is more wise in studying Man, to take our notions of humanity from those in whom it exists in the highest degree of perfection, rather than to search over-curiously for a man whose intellect is approximating to that of animals.

§ 13. In a systematic arrangement the higher forms are always to be taken before the lower.

The highest arrangement is always to be taken from the highest and most essential characters—from each highest character originates a particular section—and all the sections which are subordinate to this character are to be comprehended under its common title. The higher the distinction, the greater its dignity and importance.

Nature is always passing into series in polar opposition: hence a dichotomous mode of distribution is not only the most natural, but almost the only true one. Logic and nature, which are ever in accordance, prove this continually. Thus, for instance, natural bodies are more properly divided into organic and inorganic, than, overlooking this distinction, into minerals, plants, and animals; so also is the distribution of vegetables into cotyledoneous and acotyledoneous preferable to that of monocotyledoneous and dicotyledoneous.

But as the most sacred things are the most open to abuse, so also is the dichotomous disposition, which is of the highest value when nature is strictly followed, the most artificial of all when arbitrary distinctions take the place of those which are essential; as the analytical *index* of Lamarck. Many for this reason altogether object to such a form of arrangement; but the abuse of a thing does not destroy its use.

When the members of a bipartite section are again dichotomously divided upon analogous principles, four sections are created,

ated, of which the first and second, and the third and fourth, are in affinity; but the first and third, and the second and fourth, are in analogy.

But when this method of division becomes circuitous, a more direct path is undoubtedly to be discovered: hence other numbers are admitted, especially the quaternary (or double dichotomy), and also others in which dichotomy is understood.

There are other and most acute observers (Oken, MacLeay), who contend for other fixed fundamental numbers. Care must be taken, however, that no cabalistical or occult virtues are attributed to any particular number; in the higher spheres a higher number is, on account of the multiplicity of organs, admissible than can be used in the lower spheres; the only object of such a contrivance being to explain in what direction rays pass off from their centre, and at what points the rays of different spheres impinge upon each other. To do this a determinate number is required.

We must, moreover, avoid extending too precipitately any system whatever to specialities. We can proceed in no direction further than the power of arrangement acquired by what we positively know of nature, admits. I certainly am not of the number of those who assume that infinity is to be circumscribed within strict limits; although I may be of opinion that infinity and universal harmony are better explained by them than according to any arbitrary rules of arrangement.

In the formation of sections and genera it is most especially necessary to beware that they do not depend upon characters alone; so that if the character should hereafter prove defective, the section or the genus may still remain unchanged. In this lies the difference between an artificial and natural arrangement; the former depending upon characters, the latter upon affinity. Hence Linnæus did not characterize his families of plants, nor Ehrenberg those of fungi, rightly perceiving that affinity is of the first importance, characters of secondary.

It is occasionally necessary to admit into a particular section a genus or species in which the most important character of such section does not exist, but then its truly essential character cannot have been detected. Thus when we say that Rosaceæ are dicotyledoneous, perigynous, polypetalous, &c., and refer to them *Alchemilla*, it will be easily seen that the really essential character of Rosaceæ remains to be discovered.

§ 14. Every sphere (section) expresses a particular idea; thence its character is best expressed by a simple notion.

But to effect this, it is necessary that the character which is really most essential shall have been detected. For if a section, of which the primary character is unknown, be circumscribed by a simple notion, the most arbitrary and artificial arrangement possible would be the result.

When the essential character is once detected, all others will be wholly dependent upon it (for when this character is changed the

the others are changed also), and those which do not depend upon it are accidental.

It must not however from this be understood that a system is to be applied to one part only of its subject: on the contrary, it embraces all parts, arranging them upon the same principles,—but when they diverge in opposite directions, one is to be chosen in preference to another.

§ 15. Physical or Physiological marks are capable of distinguishing spheres (sections) of the highest order only; but in those of the lowest they are always to be consulted.

Physiological characters, as being those which are most essential, are little subject to variation, and therefore will not suffice for distinguishing the lower spheres (orders, genera, species); they are nevertheless to be continually consulted as to origin, station, geographical distribution, &c. which illustrate the series of affinities in various ways.

§ 16. Essential characters are generally the most hidden, and demand acute investigation; the most superficial being those which are accidental.

Hence it is that accidental characters, or those of a lower order are first seized, as being those which are most immediately under our eyes: thus the low distinctions of species and varieties are easily acquired by mere tyros, while the higher are within the comprehension of masters of the science alone.

The whole progress which has been made in natural history has been a succession of triumphs of the more essential characters over those accidental ones which had been previously received. Thus in the following comparison, how much more important are those distinctions which are

Essential	than those which are Superficial.
1. Mammalia, Amphibia, Pisces, of Linnæus.	1. Quadrupeds, Serpents, Fishes, of old authors.
2. Monocotyledones, Dicotyledones, &c.	2. Trees, shrubs, herbs, &c.
3. Hymenomycetes, Gasteromycetes, &c.	3. Fungi stipitati, sessiles, claviformes.
4. Lichens from their fruit.	4. Lichens from their thallus.

The foregoing proposition must not however be inverted, by supposing that *the more hidden characters are, the more essential;* Natural History would then become not only micrological, but very difficult and erroneous. Where an object is easily distinguished by marks immediately under our eyes, microscopical differences are not to be sought after. Besides, characters indicated by highly magnifying microscopes are, in fact, as superficial as those seen by the naked eye.

§ 17. The primary powers of nature are arranged according to the following laws. They are these:

A. Ter-

A. TERRESTRIAL (*Tellustres*) acting together or in contact.
a. *Acting together and continuous in their productions.*
1. *Sensibility*, or the power of motion, sensation, and consciousness. The object of *Psychology.*
2. *Vitality*, or the power of absorbing heterogeneous matter, of assimilating it to an internal circulation, and of bringing forth progeny of the same nature as the parent. The object of *Physiology.*
b. *Acting in contact, and absolute in their productions.*
3. *Affinity.* The object of *Chemistry.*
4. *Electricity.* The object of *Physics.*
B. SIDEREAL (*Siderales*) acting from a great distance.
a. Reproduction. 1. *Light.*
b. Production. 2. *Attraction.*

§ 18. The productions of nature, which are coexistent with these, are also considered as,

A. TERRESTRIAL; various in form, placed in juxtaposition or cohesion with each other, arranged both by terrestrial and sidereal influence, and composed of parts which taken together form a whole. *Natural bodies* properly so called; the objects of *Natural History.*
a. *Organic;* reproductive, composed of various definite organs, and formed by internal development.
1. ANIMALS, possessing sensation. The objects of *Zoology.*
2. VEGETABLES, possessing vitality (not sensation). The objects of *Botany.*
b. *Inorganic;* productive, homogeneous, formed of particles in juxtaposition (and not possessing the qualities of organic bodies).
3. MINERALS; ponderable. The objects of *Mineralogy.*
4. ELEMENTS; imponderable. The objects of *Physics.*
B. SIDEREAL; a system of EARTHS, which are spheroidal, very distant from each other, subject to the influence of sidereal power alone, and composed of the heterogeneous, but individually entire, productions of nature. STARS, the objects of *Astronomy.*
a. Possessing light and attraction, reproductive, central. 1. *Suns.*
b. Possessing attractive power, but no light of their own, productive, circumferential. 2. *Planets.*

VEGETABLES

are Living, insensible organic bodies.

§ 19. The end of life (and therefore of vegetation) is twofold: the preservation of the *individual* and of the *kind;* the former

former is called Nutrition, the latter Generation. Hence there is a twofold system of organs; namely, of nutrition and of multiplication. But the organs of nutrition are either prepared by the mother (*Germination*), or developed by the plant itself (*Vegetation*); so also the organs of multiplication are either confined to the plant (*Flowering*) or continued in a new individual (*Fructification*).

There are therefore four primary functions of vegetables: germination, vegetation, flowering, and fructification. This is the basis of my system.

§ 20. According to these two systems of organs, and four primary functions of life, the following arrangement is produced of

VEGETABLES.

A. Organs of Nutrition.	B. Organs of Multiplication.
a. *in Germination.*	c. *in Flowering.*
1. Cotyledoneous, *producing cotyledons.*	1. Phænogamous, *having sexes or manifest flowers.*
2. Nemeous, *producing a thread.*	2. Cryptogamous, *having no sexes, and destitute of flowers.*
b. *in Vegetation.*	d. *in Fructification.*
1. Vascular, *having cellular tissue and spiral vessels.*	1. Spermideous, *bearing seeds.*
2. Cellular, *having cellular tissue, and no spiral vessels.*	2. Sporideous, *bearing sporules.*

§ 21. Upon the same principles COTYLEDONEOUS Vegetables (Vascular, Phanerogamous, and Spermideous) are divided according to

A. Their Organs of Nutrition.	B. Organs of Multiplication.
a. *in Germination.*	c. *in Flowering.*
1. Dicotyledoneous, *with a double expanded cotyledon.*	1. (Androdynamous?)
2. Monocotyledoneous, *with a single inclosed cotyledon.*	2. (Gynodynamous?)
b. *in Vegetation.*	d. *in Fructification.*
1. Exogeneous, *the trunk youngest at the circumference.*	1. Seminiferous*. *Ag.*
2. Endogeneous, *the trunk youngest, and softest in the centre.*	2. Graniferous. *Ag.*

* Agardh defines a *Semen*, or Seed, to be
"A separate fully-formed embryo, divided into cotyledons, and with or without albumen."

A *Granum*, or Grain, he defines as "An undivided leafless embryo, ad-

§ 22. Nemeous Vegetables (Cellular, Cryptogamous, Sporideous) are also disposed according to

A. Organs of Nutrition.

a. *in Germination.*

1. Heteronemeous, *threads in germination copulating into a heterogeneous body.*
2. Homonemeous, *threads in germination either separate or confluent into a homogeneous body.*

b. *in Vegetation.*

1. Diplogeneous, *formed of regular connected cellules.*
2. Haplogeneous, *formed of anomalous somewhat filamentose cellules.*

B. Organs of Multiplication.

c. *in Flowering.*

1. Cryptandrous, *something analogous to sexual distinction.*
2. Anandrous (Link), *nothing analogous to sexual difference.*

d. *in Fructification.*

1. ? Sporiferous. Agardh*.
2. ?Sporidiiferous. Agardh.

§ 23. The Organs of Vegetation, offer modes of subdivision in proportion to the lateness of their evolution.

Germination offers very few, Vegetation a greater number, Flowers many, Fruit very numerous modes.

Their dignity is the converse of this; the most essential modes depending upon germination and vegetation, the less essential upon flowering, and almost accidental modes upon the fruit (at least the pericarpium).

In this manner the vegetable kingdom, or rather world, is divided into two hemispheres by Germination, and into four quarters by Vegetation, into Classes by Flowers, and into Orders and Families by Fructification.

§ 24. Systems truly constructed upon these principles, also comprehend all other essential differences, and at the same time explain them.

nate to an albumen which performs the functions of cotyledons, perforating the same in germination and included in a double membrane."

Of these definitions I must remark, with much respect for my very excellent friend Professor Agardh, that most of the differences he indicates between a grain and a seed are rather of words than of reality, and that many are not correct in fact. The embryo of a Monocotyledoneous seed, or as he calls it of a grain, is not adnate to the albumen, neither does it perforate that substance during germination; the membranes in which the nucleus is enveloped are the same as the membranes of other seeds, and the embryo itself has certainly no proper integuments different from those of a Dicotyledoneous embryo.

* "A *Spora* is an albuminous embryo included in a simple integument which is destitute of a hilum, and producing in germination a leaf analogous to a cotyledon (*cotyledonidium*)."

"A *Sporidium* is a naked embryo destitute both of hilum, radicle, and cotyledon." Ag. Aph. 125.

XII. *On the Ellipticity of the Earth as deduced from Experiments made with the Pendulum.* By J. Ivory, *Esq. M.A. F.R.S.*

[Continued from p. 10.]

THE figure of the earth is inferred from the relative force of gravity at different points on its surface; and therefore it depends upon the proportion of the pendulums that oscillate in a given time, and not upon their absolute lengths. Now, the proportion of two lines, nearly equal, may be known with some degree of precision notwithstanding the existence of considerable errors in the measurement of the lines themselves. If the errors be both in excess or both in defect, they will hardly alter the proportion of the lines; and if they be of opposite kinds, one in excess and the other in defect, although they will combine in changing the proportion sought, yet the variation produced will be inconsiderable, supposing that their magnitude is small in comparison of the whole lengths. When the earth's ellipticity is derived from the proportion of two pendulums found experimentally at different places, the accuracy of the result is commensurate to the accuracy with which the *ratio* of the pendulums has been determined. The error in one case will increase or decrease exactly at the same rate as the error in the other case. In independent experiments, executed with equal care, there can be no reason for preferring some and rejecting others; an equal authority must be assigned to all; and the same thing, it is evident, will be true of the results obtained when such experiments are combined with one another. This, however, must be understood as conclusive only in good combinations of the experiments, that is, in such combinations where the difference of the two pendulums is large enough to cover and, as it were, absorb the unavoidable errors of observation. In this manner of proceeding, all the results having the same weight, the most advantageous determination of the ellipticity will be found by taking the arithmetical mean, in the expectation that the excesses will compensate the defects.

Again, a minute change in the earth's ellipticity produces a great variation in the length of the pendulum; and, in consequence, a variation of the pendulum affects but slightly the quantity of the ellipticity. Hence the effect of the errors of the experiments is very different in the ellipticity, and in the general expression of the absolute length of the pendulum. The first is affected by the errors only in so far as they alter the proportion of two pendulums at distant latitudes; but, in the expression of the length of the pendulum, the errors operate

rate directly and limit the accuracy of the coefficients sought, the same quantum of error, however, having more or less influence according to the latitude of the experiment. In this latter research, therefore, it is requisite to have a number of independent experiments, and to combine them, by the known analytical methods, so as to give to each error its due weight. By this process we deduce the most advantageous formula for computing the length of the pendulum in any latitude, which, again, necessarily involves in it a mean determination of the ellipticity. Thus we may employ two different ways for finding the ellipticity of the earth from experiments with the pendulum; and consistency requires that both methods should agree in leading to the same result, more especially if in both cases the same data have been used. Our confidence in the goodness of the methods, and in the accuracy of the results, must, it is evident, be strengthened or weakened, according as they concur or fail to do so.

In what follows I shall endeavour to determine the ellipticity of the earth, by each of the two methods above mentioned separately, using the best sets of experiments with the pendulum that have hitherto been made.

1. Let e denote the earth's ellipticity; $\phi = \frac{1}{289}$, the proportion of the centrifugal force to gravity at the equator; G the gravity at the equator; and g the like force at the latitude λ: then

$$g = G\left\{1 + \left(\frac{5\phi}{2} - e\right)\sin^2\lambda\right\},$$

or,

$$e\sin^2\lambda = 1 + \frac{5\phi}{2}\sin^2\lambda - \frac{g}{G}.$$

Again, if L be the equatorial pendulum, and l the pendulum at the latitude λ; then, because the length of the pendulum is proportional to gravity, we have,

$$e^2\sin^2\lambda = 1 + \frac{5\phi}{2}\sin^2\lambda - \frac{l}{L};$$

and,

$$e = \cdot 00865 - \frac{l - L}{L\sin^2\lambda}. \qquad (1)$$

If we knew the length of the equatorial pendulum, we should thus obtain a value of e for every experiment that determined l and λ. And as the earth's ellipticity, and the total increase of the seconds pendulum from the equator to the pole, are both known within certain limits, we may derive the value of L from a pendulum found by observation very near the equator. Thus at Maranham, in latitude $2° \ 31' \ 43'' = \lambda'$, the seconds pendulum, as determined by Captain Sabine, is 39·01214 in. $= l'$; the excess of the polar above the equatorial pendulum can hardly be greater than 0·21 in., or less than 0·2 in.; wherefore,

$l' =$

$$l' = L + 0{\cdot}2 \sin^2 \lambda' = L + 0{\cdot}00039;$$

and hence, $L = 39{\cdot}01175$.

But if we are not in possession of any experiment near enough to the equator for finding L, we may still derive a value of e from any two experiments made at a sufficient distance from one another in latitude. Let λ' and λ be the two latitudes; l' and l, the two pendulums; then, we have

$$e \sin^2 \lambda' = 1 + \frac{5\varphi}{2} \sin^2 \lambda' - \frac{l'}{L},$$

$$e \sin^2 \lambda = 1 + \frac{5\varphi}{2} \sin^2 \lambda - \frac{l}{L};$$

and hence we readily get,

$$r = \frac{l}{l'},$$

$$e = {\cdot}00865 - \frac{r-1}{\sin^2 \lambda - r \sin^2 \lambda'}. \qquad (2)$$

It is evident, that (1) is only a particular case of (2), namely, when $\lambda' = 0$. But it is of more importance to notice that no reliance can be safely placed on either of the two formulæ, when the denominator on the right-hand side is a small number.

Let us now take the experiments of Captain Sabine, which give us the lengths of the seconds pendulum at thirteen stations from the equator to 80° of latitude. They are contained in the following table. The inspection of the table will show that the results it contains are very irregular near the equator, and we may extend this observation so as to include Jamaica. It must be remarked here, that there is no intention of throwing any discredit on the operations of Captain Sabine, by what is now said, or may hereafter occur in the course of discussion. I have not even seen the Captain's own work; his experiments are taken from the 39th Number of the Journal of Science; and in reasoning upon them, it must not be understood that the causes of the discrepancies occasionally noticed, are in any way alluded to. In order to have a just idea of the irregularities we may assume the equatorial pendulum as determined at St. Thomas, and 0·2 in. for the total increase from the equator to the pole, which is the least and most favourable supposition that can be made; then, having calculated from these data, the pendulums at the other stations in the table including Jamaica, the observed pendulums are uniformly short of calculation, the errors being all very great except at Ascension, and amounting to the enormous quantity of ·008 in. at Maranham and Trinidad. On the other hand, if we assume 39·01175 for the equatorial pendulum, which is the length deduced from the experiment at Maranham; and 0·2 in. for the total increase as before, this being now the

least favourable supposition, or that which gives the greatest errors; the observed pendulum at Ascension exceeds calculation no less than ·008 in.; at Trinidad the error is very small; and although the observed pendulums are greater than calculation at all the other stations, yet the errors are much smaller than they were in the case of the first equatorial pendulum. We may conclude from this comparison that, from some local anomaly which is probably explained in the original work of the author, the experiments at St. Thomas and Ascension are irreconcileable with the rest, and it will be advisable to leave them out. Beginning at Maranham we cannot obtain a good determination of the ellipticity by combining it with any of the stations, in the table till we come to New York, the variation in the length of the pendulum being too small. If we combine it with New York, and each of the five more northerly stations we obtain the results set down in the table. The calculations may be made by (2), which applies in every case; but when Maranham is combined with any other station, it will be shorter to use (1), making L = 39·01175. The six results are pretty regular, the mean of all being ·00333.

Stations.		Latitude.	Pendulum.	Elliptic.
		° ′ ″	Inches.	
	St. Thomas	0 24 41 N.	39·02074	
	Maranham .	2 31 43 S.	39·01214	
	Ascension . .	7 55 48 S.	39·02410	
	Sierra Lieone	8 29 28 N.	39·01997	
	Trinidad . .	10 38 56 N.	39·01884	
	Bahia	12 59 21 S.	39·02425	
	Jamaica . . .	17 56 7 N.	39·03510	
Maranham and	New York .	40 42 43	39·10168	·00323
Maranham and	London. . .	51 31 8	39·13910	·00332
Maranham and	Drontheim .	63 25 54	39·17456	·00343
Maranham and	Hammerfest	70 40 5	39·19519	·00337
Maranham and	Greenland .	74 32 19	39·20335	·00336
Maranham and	Spitzbergen .	79 49 58	39·21469	·00328
			Mean . .	·00333
New York and Spitzbergen				·00332
Sierra Leone and . .	Greenland			·00347
Sierra Leone and . .	Spitzbergen . . .			·00338
Trinidad and	Greenland			·00337
Trinidad and	Spitzbergen . . .			·00328
Bahia and	Greenland			·00344
Bahia and	Spitzbergen . . .			·00334
Jamaica and	Greenland			·00348
Jamaica and	Spitzbergen . . .			·00339

In

In the latter part of the table the results of some other combinations of the experiments are set down. In particular Sierra Leone, Trinidad, Bahia, and Jamaica, are each combined with Greenland and Spitzbergen, the most northerly stations, these being the cases in which the total variation of the pendulum is greatest and most likely to cover the irregularities of observation. We may conclude, that in no good combination of the experiments, will the ellipticity come out greater than ·00349. We may safely adopt, as the true mean ellipticity, the quantity ·00333 obtained from the first six combinations in the table.

Captain Kater has determined the length of the seconds pendulum at seven different stations between Dunnose in the Isle of Wight, and Unst the most remote of the Shetland islands, extending through 10° of latitude. The total variation of the pendulum between the extreme stations is only ·035 in., which is much too small for safely computing the earth's ellipticity, on account of the great irregularities of observation to which such operations are liable. But although Captain Kater's experiments, when taken by themselves, are insufficient for determining the ellipticity of the earth, they are not the less valuable, because they may be combined with the experiments of others made at a proper distance from them. The following table contains the ellipticities obtained by combining the length of the pendulum determined by Captain Sabine at Maranham, with Captain Kater's experiments. The pendulum at Trinidad combined with the same experiments will give very nearly the same results.

Stations.	Latitude.	Pendulum.	Ellipticity.
	° ′ ″	Inches.	
Unst	60 45 28 N.	39·17146	·00329
Portsoy	57 40 59	39·16159	·00327
Leith Fort . . .	55 58 41	39·15554	·00328
Clifton	53 27 43	39·14600	·00332
Arbury Hill . .	52 12 55	39·14250	·00328
London	51 31 8	39·13929	·00332
Shanklin Farm	50 37 24	39·13614	·00331
		Mean . .	·00329

In the article "*Pendulum*" in the Supplement to the Encyclopædia Britannica, M. Biot has given us the lengths of the pendulum as determined by himself and other French philosophers at eight different stations, extending through 22° of latitudes from Formentera to Unst. The seconds pendulum is expressed

expressed in millimetres, and it refers to the decimal division of the day. The total variation of the decimal pendulum between the extreme stations is 1·74 mm., and to this quantity the unavoidable errors of observation bear too great a proportion to admit of many safe determinations of the ellipticity by different combinations of the experiments. In order to combine these experiments with that of Captain Sabine at Maranham, it is necessary to reduce the sexagesimal pendulum in English inches, or rather the equatorial pendulum 39·01175 in. deduced from it, to millimetres and the decimal division of the day. Now when the sexagesimal pendulum is 39·01175 in., it will be found that the decimal pendulum is $739^{mm}.6885$, which is therefore the value of L in the formula (1) when it is applied to the French experiments. The resulting ellipticities are contained in the following table.

Stations.	Latitude.	Pendulum.	Ellipticity.
	° ′ ″	mm.	
Formentera . . .	38 39 56 N.	741·2520	·00324
Figeac	44 36 45	741·6122	·00338
Bourdeaux . .	44 50 26	741·6087	·00343
Clermont . . .	45 46 48	741·7052	·00334
Paris	48 50 14	741·9175	·00333
Dunkirk	51 2 10	742·0770	·00331
Leith Fort . . .	55 58 37	742·4134	·00329
Unst	60 45 25	742·7231	·00326
		Mean	·00332

The result of all these calculations stands thus:

Mean of Captain Sabine's experiments . . . ·00333
of Captain Kater's . . . . ·00329
of M. Biot's . . . . ·00332

2. We next proceed to the other method of determining the earth's ellipticity. As before, let L represent the equatorial pendulum; f the whole increase from the equator to the pole; l' the observed length of the seconds pendulum at the latitude λ', and $l' + \varepsilon$ the true length, ε being the error of observation: then we have

$$l' + \varepsilon = \text{L} + f \sin^2 \lambda',$$

$$f = \left(\frac{5\varphi}{2} - e\right) \times \text{L}.$$

Again, let l be another observed pendulum at the latitude λ, and $l + \varepsilon + x$, the true pendulum, x being the difference of the errors at the latitudes λ and λ'; then,

$$l + \varepsilon + x = \text{L} + f \sin^2 \lambda.$$

 Thus

Thus we get, $$\left.\begin{aligned} L &= l' + \varepsilon - f\sin^2\lambda' \\ x &= f(\sin^2\lambda - \sin^2\lambda') - (l - l'). \end{aligned}\right\} \quad (3)$$

Taking the whole of Captain Sabine's experiments, except those at St. Thomas and Ascension, suppose that l' and λ' are the experimental quantities at Maranham, and for l and λ substitute successively the experimental quantities at the several stations in the order in which they stand in the foregoing table; then, observing that we have before found $f\sin^2\lambda' = \cdot00039$, we shall get these equations,

$$\begin{aligned} L &= 39{\cdot}01175 + \varepsilon \\ x^{(1)} &= \cdot01985\,f - \cdot00783 \\ x^{(2)} &= \cdot03220\,f - \cdot00670 \\ x^{(3)} &= \cdot04857\,f - \cdot01211 \\ x^{(4)} &= \cdot09288\,f - \cdot02296 \\ x^{(5)} &= \cdot42349\,f - \cdot08954 \\ x^{(6)} &= \cdot61085\,f - \cdot12696 \\ x^{(7)} &= \cdot79800\,f - \cdot16242 \\ x^{(8)} &= \cdot87827\,f - \cdot18305 \\ x^{(9)} &= \cdot92698\,f - \cdot19121 \\ x^{(10)} &= \cdot96689\,f - \cdot20255 \end{aligned}$$

As these equations of condition are different from those usually employed in this research, it is necessary to explain the reason of departing from the common method. The aim is to find a value of L and one of f, that will give the observed length of every pendulum exactly if it can be done, otherwise as nearly as possible. Now, we should be able to find values of L and f that would satisfy the case of every pendulum, if all the quantities $x^{(1)}$, $x^{(2)}$, $x^{(3)}$ &c. were evanescent, so that every one of the equations gave the same value of f; for then the expression of L would contain no unknown quantity, except ε, which would be determined by the particular case of any one of the pendulums. But, on account of the irregularities to which the experimental quantities are liable, the equations do not all give the same value of f, and the quantities $x^{(1)}$, $x^{(2)}$, &c. are not all equal to zero; and we must therefore proceed in the usual way to find the most advantageous value of f, by making the sum of the squares of $x^{(1)}$, $x^{(2)}$, &c. a minimum. When f is found, the expression of L will still contain the arbitrary quantity ε, which may be determined by a particular pendulum, or by any other consideration deemed preferable. In the usual method of proceeding the quantities L and f are found by investigating the minimum of the expression,

$$\varepsilon^2 + (\varepsilon + x^{(1)})^2 + (\varepsilon + x^{(2)})^2 + \&c.,$$

that is, by making the sum of the squares of all the errors a minimum.

minimum. Now, in this it is evidently assumed that the conditions of the problem can be exactly fulfilled in no other case except the contemporaneous evanescence of all the errors. But this supposition is too limited; for the conditions will be exactly fulfilled when the errors are all equal to the same quantity ε, although they be not all evanescent. What obstructs the perfect solution of the problem is the circumstance that $x^{(1)}$, $x^{(2)}$, $x^{(3)}$, &c. are not all equal to zero; and we shall approach the nearest to a perfect solution when we assign to these quantities the least values that the case will admit of.

Applying the usual rule to the foregoing equations of condition, we get this equation for finding f, viz.

$$0 = 3{\cdot}76723 f - 0{\cdot}78207,$$

the coefficient of f being the sum of the squares of all the coefficients of f in the several equations, and the other number, the sum of the products of the two numbers in each equation. Hence we get $f = 0{\cdot}2076$; and the formula for l, the length of the pendulum at any latitude λ, is

$$l = 39{\cdot}01175 + \varepsilon + 0{\cdot}2076 \sin^2 \lambda.$$

We may determine the arbitrary quantity ε in different ways. If, in a set of experiments, any one is entitled to much greater confidence than the rest, ε may be determined so as to make the error of that experiment equal to zero: and, if there be no reason for preferring one experiment to another, ε may be determined so as to make the sum of all the errors evanescent. The following table contains the pendulums calculated by the formula on the supposition that $\varepsilon = 0$, together with the excesses of the calculated above the observed quantities.

Stations.	Calculated Pendulums.	Excess of Calculation.
	inches.	
Maranham . . .	39·01214	·00000
Sierra Leone . .	39·01627	− ·00370
Trinidad	39·01884	·00000
Bahia	39·02223	− ·00202
Jamaica	39·03143	− ·00367
New York	39·10006	− ·00162
London	39·13896	− ·00015
Drontheim . . .	39·17780	+ ·00324
Hammerfest . .	39·19659	+ ·00140
Greenland	39·20459	+ ·00124
Spitzbergen . . .	39·21288	+ ·00181

As the error is very small at London, there seems to be no

good reason for expecting to better the formula by seeking a more advantageous value of ε. With regard to the ellipticity resulting from this combination of the experiments, we have,

$$e = \cdot 00865 - \frac{f}{L} = \cdot 00333$$

which is precisely the mean quantity we found by the first method.

If we join Captain Sabine's experiment at Maranham to those of Captain Kater, taken in the order in which they are set down in the foregoing table, and proceed in the same manner, we shall get these equations, viz.

$$\begin{aligned} L &= 39\cdot 01175 + \varepsilon \\ x^{(1)} &= \cdot 75942 f - \cdot 15932 \\ x^{(2)} &= \cdot 71225 f - \cdot 14945 \\ x^{(3)} &= \cdot 68500 f - \cdot 14340 \\ x^{(4)} &= \cdot 64360 f - \cdot 13386 \\ x^{(5)} &= \cdot 62265 f - \cdot 13036 \\ x^{(6)} &= \cdot 61085 f - \cdot 12715 \\ x^{(7)} &= \cdot 59557 f - \cdot 12400 \end{aligned}$$

And hence,

$$\begin{aligned} o &= 3\cdot 08299 f - \cdot 64451 \\ f &= 0\cdot 2091, \\ l &= 39\cdot 01175 + \varepsilon + 0\cdot 2091 \sin^2 \lambda. \end{aligned}$$

The following table contains the comparison of this formula with observation, ε being supposed equal to zero.

Stations.	Calculated Pendulum.	Excess of Calculation.
	inches.	
Unst	39·17088	— ·00058
Portsoy	39·16103	— ·00056
Leith Fort . . .	39·15532	— ·00022
Clifton	39·14668	+ ·00068
Arbury Hill . .	39·14230	— ·00020
London	39·13983	+ ·00054
Shanklin Farm .	39·13663	+ ·00049

Here the errors are very small. And it appears from both methods of calculation that, in this set of experiments, the discrepancies are contained within very narrow limits. The resulting ellipticity is ·00329, which is exactly the mean quantity found by the first method.

As this second method of calculation has been found to agree exactly with the first in two sets of experiments, we may presume that there will be the same perfect agreement in the third set. To save calculation we may, therefore, deduce

duce the length of the pendulum in M. Biot's experiments from the mean ellipticity already found. Now, $e = \cdot 00332$, $L = 739\cdot 6885$ mm.; and, hence

$$f = \left(\frac{5\phi}{2} - e\right) \times L = \cdot 00533 \times L = \overset{\text{mm.}}{3\cdot 9425}:$$

consequently, $l = \overset{\text{mm.}}{739\cdot 6885} + \overset{\text{mm.}}{3\cdot 9425} \times \sin^2 \lambda.$

The following table shows the differences of the pendulums calculated by this formula and the experimental quantities.

Stations.	Calculated Pendulums.	Excess of Calculation.
	mm.	
Formentera . . .	741.2264	— ·0256
Figeac	741·6331	+ ·0208
Bourdeaux . . .	741·6488	+ ·0401
Clermont	741·7134	+ ·0082
Paris	741·9230	+ ·0055
Dunkirk	742·0720	— ·0050
Leith Fort . . .	742·3967	— ·0167
Unst	742·6901	— ·0330

M. Biot has investigated the general expression of the length of the pendulum from the ellipticity adopted by Laplace, and from the length of the pendulum observed by himself at Unst which is assumed as free from error. The differences between the formula and the experiments are given in the article already cited; they are all on one side, except at Formentera; and they pass a little beyond the limit of the greatest error in the table above.

From the whole of this discussion it appears that the ellipticities deduced from the three sets of experiments approach to one another, and to the like quantities found by other methods, nearer than could have been expected, considering the irregularities to which the operations are liable. Capt. Sabine has, however, inferred a much greater ellipticity, equal to 0·00346, from an extensive comparison of the same experiments; but as I have not seen his work, I can give no opinion respecting the processes on which his conclusion is founded.

Aug. 4, 1826. J. Ivory.

XIII. *Experi-*

XIII. *Experiments proving that* Mariotte's *Law is applicable to all Kinds of Gases; and to all Degrees of Pressure under which the Gases retain their aëriform State.* By H. C. Oersted*.

THE law of Mariotte†, according to which the spaces occupied by a certain quantity of gas or air are found to be in an inverse ratio of the degrees of pressure which they suffer, has hitherto been demonstrated by strict experiment for very small degrees of pressure only. Several men of science of the first class have assumed this law as being applicable to every degree of pressure, in exact conformity with nature: others, and among these Jacob Bernoulli and Euler, entertained the opinion that the spaces decrease in a smaller progression than that in which the pressure is increased; and if, in fine, we refer to the small number of experiments made with high powers of pressure, the proportions of space seem to decrease in a much greater progression than that in which the pressure increases. Sulzer, a well-known German writer, has published, in the Transactions of the Academy of Berlin, experiments advancing to a pressure of eight atmospheres. Robison, a very respectable English writer, made similar experiments.—The following table furnishes the results obtained by both.

Sulzer's Experiments, (complete series).		Robison's Experiments with dry air.	
Density.	Compressing Powers.	Density.	Compressing Powers.
1·000	1·000	1·000	1·000
1·091	1·076	2·000	1·957
1·200	1·183	3·000	2·848
1·333	1·303	4·000	3·737
1·500	1·472	5·500	4·930
1·714	1·659	6·000	5·342
2·000	1·900	7·620	6·490
2·400	2·241		
3·000	2·793		
4·000	3·631		
6·000	5·297		
8·000	6·835		

* Read before the Royal Society of Copenhagen, and published in Schweigger's Journal, N.R. Band xv. p. 352.

† It is well known that this law was first deduced from the experiments of the famous Boyle, by his friend Richard Townley: nevertheless I have called it by the name of Mariotte, who discovered it at the same time by an experiment of his own, as it is commonly known by this appellation.—O.

Captain

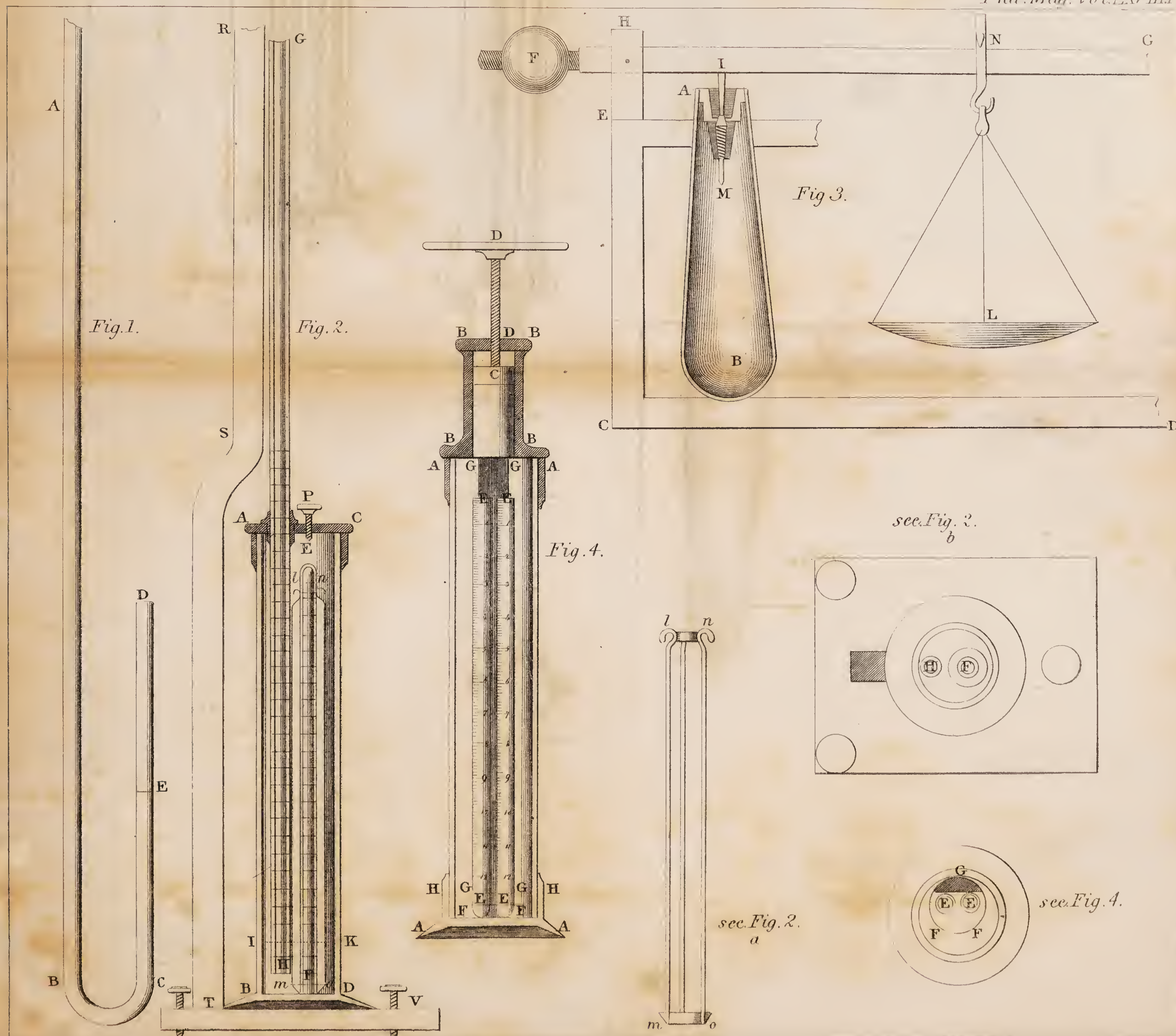
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
see Fig. 2. b
sec. Fig. 2. a
see Fig. 4.
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Captain Schwendsen and myself, desirous of making some experiments on the air-gun, felt the necessity of first trying the extent of the Mariottic law, to serve us for a basis. The apparatus generally used for this purpose is known to consist of a curved tube ABCD (Pl. II. fig. 1.) one part of which DE contains the air, and the other ABCE mercury; the object of which is to inclose and compress the air. This apparatus, however, has several inconveniences: it is difficult to subdivide the part DE of the tube into equal spaces; besides, this part is extended by the pressure within, and there is some risk of its bursting if the pressure becomes too great. To counteract such an accident, tubes of a smaller diameter are used; but this expedient again creates a friction sufficient to disturb the results to a considerable degree. To avoid, therefore, all these inconveniences, we had recourse to an apparatus which, constructed after the same principle, had formed part of my apparatus for the compression of water. Fig. 2. presents a vertical section of this new apparatus. ABCD is a very strong glass cylinder, having a brass lid. EF is a graduated glass tube supported by an iron frame *l m n o*, which at its lower end terminates in an iron cup containing some mercury. This closes the tube EF before it is plunged into the mass of mercury spread at the bottom of the cylinder. IK shows the superior limit of the quicksilver. GH represents a part of a very strong glass tube, which is cemented into a hollowed piece of metal, part of the outer surface of which is adapted to a nut which is in the lid of the cylinder. The two additional figures marked by *a* and *b* in fig. 2. represent, one the frame *lmno*, and the other the transverse section of the lower part of the apparatus. If an experiment is to be made with this apparatus, the lid AC is screwed off; the tube EF, filled with carefully dried air, is plunged into the cylinder, the lid screwed on again, and well secured. Then the tube GH is also put in its place, and the cylinder is filled with water by means of a funnel placed in the aperture P. The pressure produced by this is measured by the rising of the mercury in the tube GH. Finally, the apparatus is closed by means of the screw fitted to the aperture P, and mercury poured into the tube GH, which also rises in the tube EF, and compresses the air contained in it. The distance of the levels of the mercury in the tubes EF and GH, both being graduated alike, show by simple subtraction the magnitude of the compressing power. The tube EF is nearly throughout of equal size; nevertheless we have determined the spaces corresponding with the divisions by means of quantities of mercury carefully weighed. The graduation of the tube GH extends only a few inches above

above the cylinder, the other distances having been measured, by means of a separate scale.

The tube GH was lengthened for high pressures by our joining together several tubes seven feet in length or more, by means of iron screws. The experiment was always made in the hall near the stairs of the house containing the cabinet of natural philosophy of the university; there being no room high enough for the lengthened tube GH.

Several of the experiments we made, agreed in their results with the Mariottic law: but a similar success did not attend the whole of them, owing to the difficulty of making all the cemented joints and screws resist the great pressure which forces the mercury into them.

In one experiment only, the results of which we shall communicate immediately, we were enabled to carry the pressure to as many as eight atmospheres. The air contained in the tube EF had been well dried by chloride of calcium; the space inside the tube, measured by mercury, amounted to 1054·8 grammes at 20° Cent.; the pressure of the atmosphere on the day of the experiment =0·7578 of a metre height of the mercury. The following table shows the proportions which we found between the compression of the air and the pressure of the mercury. The first column contains the degrees of density of the original volume of air divided by the quantities of space proportionally diminished by the compressing powers;—the second shows these powers in figures, assuming the pressure of the atmosphere on the day of experiment as *one;*—the third shows the differences between the different degrees of density and compressing powers;—and the fourth shows the proportion borne by those differences to the compressing powers.

Density.	Compressing Powers.	Differences.	Differences divided by the compressing Powers.	Density.	Compressing Powers.	Differences.	Differ. divided by compress^g Powers.
1·000	1·000	·0000	·0000	3·168	3·147	+0·021	+0·007
1·1052	1·1051	+0·0001	+0·0001	3·616	3·599	+0·017	+0·005
1·1676	1·1693	−0·0017	−0·0015	4·209	4·185	+0·024	+0·006
1·2763	1·2706	+0·0030	+0·0024	5·057	5·010	+0·047	+0·009
1·4744	1·4694	+0·0050	+0·0035	5·603	5·572	+0·031	+0·005
1·587	1·581	+0·006	+0·004	6·288	6·287	+0·001	+0·000
1·812	1·806	+0·006	+0·003	7·725	7·082	+0·093	+0·013
2·112	2·079	+0·033	+0·016	8·030	8·014	+0·016	+0·002
2·529	2·520	+0·009	+0·004				

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It is very difficult in these experiments to determine with accuracy the volume of the inclosed column of air, it being limited below by a curved plane, the form of which varies according to the friction between the mercury and the glass. We endeavoured, however, to divide this curve by the eye, into two equal portions; nevertheless it is proved by the result that we calculated the inclosed air too low. Without this error, the differences would have been smaller, and the numbers sometimes higher, sometimes lower. But still the differences are smaller than could have been expected in experiments which preclude the use of the vernier.

In the last experiments, for instance, the height of the observed column of air amounted to 56·4 millimetres; according to the Mariottic law, it should have been 56·287; the whole difference, therefore, is no more then 0·113 millimetres, an error which in such experiments is quite inevitable. In the preceding one, the height of the observed column was =63·17 millimetres, which, according to the Mariottic law, should have been =63·99. This difference, the greatest we have had, amounts to 0·82 millimetre; but being between two observations which offer but a very small deviation, it will not militate against the general law.

In order to investigate the compression of the air by greater powers, we made use of the air-gun: the King, whose enlightened generosity has already done so much for the advancement of science, having placed all the requisite apparatus at our disposal. Every one knows that in this kind of weapon, it is the but-end which is the reservoir of the compressed air. This part, therefore, is always made very strong. We first ascertained the inner space of one by weighing it, first when empty; and again when filled with water. By this means, the quantity of air that might be contained in it, could be easily ascertained. The one which we used most frequently, could receive 0·891 grammes of air at a height of 0·76 metres of the mercury of the barometer. We were also enabled to determine by weight the degree of density we obtained in our experiments; a method which proved sufficiently accurate, as the scales we made use of were affected by one centigramme. We succeeded in forcing into one but-end, 101·2 grammes of air, a quantity answering to the pressure of 110·5 atmospheres. We also took into consideration the expansion caused by the pressure from within on the reservoir, and determined it by weighing in water the but-end empty, and filled with air. In calculating, we assumed, that the different degrees of this ex-

 pansion

pansion are in proportion to the quantities of air which had been put into it. If the reservoir had not been extended after 101·2 grammes of air had been forced into it, the density of this air would have equalled 113·5 of the atmosphere; but taking into consideration the expansion of the reservoir, it only amounted to 110·5.

The third figure shows the manner in which we carried on our experiments on the expansive power of the air compressed in such a reservoir. AB represents such a reservoir, *i. e.* the but-end of an air-gun; CD is a board with a perpendicular lath CE; EH is a piece of iron which in its upper part receives the axis, round which turns the lever FG, which again is balanced by a counterpoise, F. This lever has a tooth I, pressing on the valve M, of the but-end MB fastened underneath it. A slide N with a scale L appended to it, serves to determine the power necessary for opening the valve. This valve being closed by a spring, we began by investigating the power necessary to open the valve when the density of the inclosed air corresponded with that of the atmosphere. After this the reservoir was loaded as much as possible; and after having measured the resistance occasioned by the inclosed air against the valve, we gradually emptied the reservoir, constantly weighing the remaining air by means of scales and weights, and determining its expansive power by means of the apparatus fig. 3. Experiments of this kind, however, are not susceptible of great accuracy, since the valve does not always close uniformly. If the valve is lined with leather, for the purpose of making it close more tightly, that inequality is very great; for which reason we began a series of experiments with a steel valve closely ground-in, which, however, prevented us from obtaining equally strong charges.

The following tables give the result of both experiments, the first column of which shows the air forced into the reservoir; the second, its density; the third, the force requisite for opening the valve,—that which was necessary before loading being deducted; and the fourth, the pressure of the atmosphere, being the product of the magnitude of this power divided by the degrees of density.

Table

Table I.

Experiments with a but-end having a valve lined with leather.

Weight of the inclosed air, in grammes.	Density, that of the atmosph. =1.	Pressure on the valve, in grammes.	Pressure, divided by the density.	Weight of the inclosed air, in grammes.	Density, that of the atmosph. =1.	Pressure on the valve, in grammes.	Pressure, divided by the density.
1	1·122	812	725	8	8·960	6797	758
2	2·243	1809	806	9	10·077	7711	764
3	3·364	2552	758	10	11·193	8166	729
4	4·484	3693	823	10	11·193	8434	753
5	5·604	3495	784	10	11·193	8480	757
6	6·723	5750	855	10	11·193	8445	754
7	7·842	6693	853	10	11·193	8437	753

Table II.

Experiments with a but-end, the valve of which was without leather.

Weight of the inclosed air, in grammes.	Density, that of the atmospher. =1.	Pressure on the valve, in grammes.	Pressure, divided by the density.	Weight of the inclosed air, in grammes.	Density, that of the atmospher. =1.	Pressure on the valve, in grammes.	Pressure, divided by the density.
1	1·122	1269	1131	10	11·193	11440	1022
2	2·243	2368	1055	10·2	11·417	11725	1027
3	3·364	3388	1007	15	16·76	16766	1000
4	4·484	4751	1059	15·1	16·87	17243	1022
5	5·604	5750	1026	20	22·326	22988	1029
5	5·604	5620	1002	25·6	28·543	29253	1025
5·05	5·657	5790	1023	30	33·393	34197	1024
5·05	5·657	5800	1025	35·2	39·13	40232	1026
5	5·604	5790	1022	40·1	44·52	45633	1025
6	6·732	6871	1021	45	49·894	57641	1035
7	7·842	8113	1034	50	55·362	57467	1038
8	8·960	9344	1043	55	60·816	63102	1037
9	10·077	10375	1029	60	66·254	67798	1023

In the first table the mean number is 797, and we find that the deviations from it are not regular. The mean of the second table is 1027 (by excluding the first number, the deviation of which is too great), and we also find that most of the numbers do not deviate far from it. However imperfect these

 experiments

experiments may be, and must be from their very nature, they contribute to prove that the compressions produced by very great powers, are regulated by the same laws as those produced by very low powers. But wishing to ascertain whether the compression of every kind of gas was subject to the same law, we selected such of the gases as are liable by the pressure of a few atmospheres to be changed into a dewy liquid, particularly the sulphurous acid gas, which, (according to Faraday,) becomes such a liquid under the pressure of two atmospheres.

Two glass tubes of equal sizes, the one filled with well dried sulphurous acid gas, the other with atmospheric air, were placed in a small basin of mercury and put into an apparatus by means of which these gases could be exposed to the proper pressure. The result was, that both volumes were diminished in a constantly equal ratio, till the moment when the sulphurous acid gas began to liquefy.

We add the following details of these experiments:

AAAA (fig. 4.) is a very strong glass cylinder, the same which I use for the compression of water. This cylinder has a brass lid; on this is raised another cylinder BBBB, in which a piston C is moveable up and down through the aperture DD. EEEE are two equally graduated tubes, the lower extremities of which are fixed in a small iron basin FF. This basin is fastened to a strip of glass GGGG, which serves at the same time to keep the tubes in a perpendicular position. The cylinder AAAA is filled with mercury up to HH. The experiment is begun by the two tubes being filled with the gases, put into the small basin, and fastened to the strip of glass GGGG. The whole apparatus is then placed in the cylinder AAAA, by which the basin is plunged into the mercury below the line HH; the cylinder is then filled with water, the pump-cylinder BBBB, put upon it, also filled with water, and the piston made to press on the inclosed water. The water communicates the pressure to the mercury, which again transfers it to the gases in the tubes. Fig. 4. presents a transverse section of the lower part of the apparatus.

Experiments

Experiments made with two tubes, the one filled with atmospheric air, the other with sulphurous acid gas. The temperature was $21\frac{1}{4}$° *Cent.*

Sulphurous acid gas.	Atmospheric air.	Compression of the gas.	Compression of the atmosph. air.	Differences.
131·2	128·5	1·	1·	
128·	125·33	1·0261	1·0259	+0·0002
122·4	120·	1·0754	1·0768	−0·0014
117·33	115·	1·1229	1·1215	+0·0014
112·	110·	1·1750	1·1729	+0·0021
106·875	105·	1·2302	1·2297	+0·0005
101·5	100·	1·2937	1·2942	−0·0005
96·3	95·	1·3634	1·3644	−0·0010
91·25	90·	1·4396	1·4403	−0·0007
86·	85·	1·5278	1·5257	+0·0021
80·75	80·	1·6228	1·6228	0·000
75·5	75·	1·7329	1·7511	+0·0018
70·6	70·	1·8542	1·8539	+0·0003
65·6	65·	1·9971	1·9974	−0·0003
64·5	64·	2·0310	2·0307	+0·0003
63·4	63·	2·0649	2·0638	+0·0011
62·4	62·	2·0976	2·0982	−0·0006
61·3	61·	2·1342	2·1336	+0·0006
60·3	60·	2·1705	2·1702	+0·0003
59·25	59·	2·2101	2·2082	+0·0019
58·2	58·	2·2475	2·2474	+0·0001
57·16	57·	2·2879	2·2874	−0·0005
56·	56·	2·3356	2·3289	+0·0067
54·875	55·	2·3835	2·3720	+0·0115
53·875	54·	2·4279	2·4166	+0·0113
52·8	53·	2·4798	2·4629	+0·0169
51·75	52·	2·5317	2·5105	+0·0268
50·6	51·	2·5831	2·5610	+0·0221
49·6	50·	2·6488	2·6171	+0·0317
48·6	49·	2·7008	2·6674	+0·0434
47·6	48·	2·7595	2·7240	+0·0355
46·6	47·	2·8207	2·7819	+0·0388
45·5	46·	2·8886	2·8423	+0·0463
44·4	45·	2·9556	2·9057	+0·0499
43·33	44.	3·0240	2·9717	+0·0523
42·4	43·	3·0974	3·0407	+0·0567
41·16	42·	3·1733	3·1130	+0·0603
39·33	41·	3·3186	3·1889	+0·1297
34·5	40·	3·7796	3·2689	+0·5080
20·33	39·	3·4890	3·3526	+0·1364

This table shows that the differences are very small, and that sometimes one, and sometimes the other of the gases, suffers a greater degree of condensation, up to a pressure of 2·3 atmospheres, where they become greater, and the sulphurous acid gas shows continually a superior density. At a pressure of 3·2689 the moisture begins to be visible, and the condensation shows itself in a much more violent and decided manner. Perhaps some small liquefaction takes place before it reaches this point, where the gases come in contact with the glass and the mercury, for the contact with a heterogeneous body seems to favour the transition from one state of aggregation into another, as I have shown in a former treatise on some experiments of Winterl's*.

In some experiments we found that the water had penetrated between the sides of the tubes and the mercury. This inconvenience was subsequently obviated by our cementing the extremity of the tube into a brass ring, which amalgamates with the mercury, and prevents the water from penetrating†.

We also compressed cyanogen in the same manner, and found that the liquefaction of this gas begins when the air has been compressed to 1-3·5 of its weight, at 23° of heat, and a state of the barometer of 0·759 height of the mercury.

It would be easy to multiply these experiments; but those we have just communicated will, we trust, suffice to prove that the compression of atmospheric air and of gases, is in proportion to the compressing forces, however great the latter may be, supposing that the gases remain in their aëriform condition, and that the caloric liberated by the compression has been carried off. Thence it appears, that our investigations have done no more than confirm the opinions of the most distinguished men of science of our time with respect to this subject; but as there still remained some who enter-

* Vide Gehlen's *Journal d. Phys. und Chem.* 1806, vol. i. p. 276—89.

† The means by which Professor Oersted thus succeeded in removing this source of error, appears in some degree to confirm the results obtained by Mr. Daniell, in securing barometers from the deterioration ensuing from the insinuation of *air* between the glass and the mercury at the lower end of the tube. Mr. D. found, that welding a cylinder or a ring of platinum to the extremity of the tube immersed in the mercury, effectually prevented this evil; in the same manner as the insinuation of *water* between glass and mercury was prevented by the Danish philosopher. Dr. Priestley also found that the admission of air and of water into jars of gas confined over mercury, was prevented by a little water poured around the jar *upon* the mercury; and Mr. Faraday has ascertained that gases unconfinable over mercury, may be preserved over water. The effect, in all these cases, is attributable to the perfect contact ensuing from the juxta-position of substances capable of acting *chemically* on each other.—E. W. B.

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tained a contrary opinion, we considered that the publication of our experiments might not prove altogether useless.

The compression of liquid bodies reducible to drops, is, as far as our experience yet goes, subject to the same law. Here too, the compression and the compressing power seem to bear a direct relative proportion. We may therefore assume, that the gases converted into liquids reducible to drops, begin again to follow the same law to which they answered as gases. If this should be confirmed by further experiments, it may be said that the compression of a body ceases to conform to these rules, only in its moment of transition from one state of aggregation to another.

XIV. *On the Adhesion of Glue.* *By* B. Bevan, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

EXPERIMENTS, when carefully made on the properties of substances which are daily used in large quantities, cannot be altogether uninteresting, but may lead to improvements in the arts.

Having for some years been in possession of apparatus for measuring with accuracy mechanical forces to a considerable extent, I avail myself of a leisure hour, when I can find one, of trying various elementary experiments. I have lately tried the force of adhesion of *common glue.* The method I adopted was that of gluing together two cylinders of dry ash wood, of 1·5 in. diameter, and about eight inches long: these were submitted to the lever apparatus after being glued together twenty-four hours. To separate the cylinders required a force of 1260 pounds; and as the area of the circular ends of the cylinders were 1·76 in., it follows that the force of 715 pounds would be required to separate *one square inch.* It is right to observe that the glue used in this experiment was fresh made, and the season very dry. Upon referring to some former experiments on this substance, made in the winter season, and upon glue which had been frequently melted, with occasional additions of glue and water, I obtained a result of 350 to 560 pounds to the square inch. My present experiment was conducted upon a larger scale, and with greater care in the direction of the resultant of the force, so that it might be, as near as was practicable, in a line passing at right angles, through the centres of the surfaces in contact; the pressure was applied gradually, and was sustained two or three minutes before the separation took place.

Upon

Upon examining the separated surfaces, the glue appeared to be very thin, and did not entirely cover the wood, so that the actual cohesion of glue must be something greater than 715 pounds to the square inch.

I also tried the lateral cohesion of fir-wood, from a Scotch fir of my own planting, cut down in the autumn of 1825, and sawn into boards, being at the time of this experiment quite dry and seasoned. The force required to separate the wood was 562 pounds to the square inch; so that if two pieces of this wood had been well glued together, the wood would have yielded in its substance before the glue.

The cohesion of Memel fir, across the grain, I have found to be from 540 to 840 pounds to the square inch, on a specimen tried some time ago, the modulus of elasticity of which in the same position of the grain, varied from 40,500 to 44,600 pounds; and the modulus of elasticity of the Scotch fir timber used in the experiment above described, was 24,600 pounds.

I also tried the force required to break, or tear out, a half-inch iron pin, applied in the manner of a pin to a tenon in the mortice; the thickness of the board being 0·87 in. and distance of the centre of the hole from the end of the board 1·05 in.;—the force required was 976 pounds. As the strength of a tenon, from the pin-hole, may be considered in proportion to the distance from the end, and also as the thickness; we may, for this species of wood, obtain the breaking force in pounds nearly, by multiplying together one thousand times the distance of the hole from the end, by the thickness of the tenon in inches. I am, dear sir, yours truly,

B. Bevan.

P.S. From an experiment subsequently made on *solid glue*, I find the cohesion to be 4000 pounds to the square inch, from which it may by inferred that the application of this substance as a cement is susceptible of improvement. B. B.

XV. *Researches on the Theory of Hydro-dynamics. By* Thomas Tredgold, *Esq. Civil Engineer.*

[Second Communication.]

Prop. 1.—The resistance to the motion of a body in a fluid is equal to the quantity of motion it impresses on the fluid.

For, if the quantity of motion were greater than the resistance, the effect would be greater than the power producing it, and if it were less, the resistance would be greater than the resisting forces.

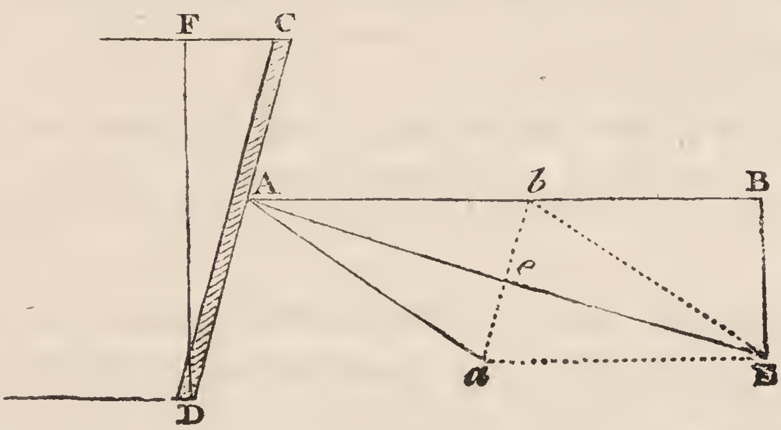
Prop.

Prop. 2.—The quantity of motion impressed on a fluid by a body moving in it must be estimated in a direction perpendicular to the resisted surface, whatever may be the direction of the motion of the body.

The resistance on a surface is composed of the force which moves the fluid in the direction of its motion, and the resistance to reflection; the resultant of these forces is perpendicular to the surface; but the whole action to displace the fluid being measured by the resultant, the velocity of displacement must be estimated in the same direction: hence the quantity of motion impressed being in the direction of the resultant is perpendicular to the surface.

Prop. 3.—The direct velocity of a body is to the velocity of any part of that body in a direction perpendicular to its surface as the radius is to the sine of the angle of inclination of the surface to the direction of its motion.

Let A*b* represent the direction and velocity of the plane, and draw A*e* perpendicular to the plane, and *b e* parallel to



the plane, then A*e* is the velocity in a direction perpendicular to the plane; and A*b* is to A*e* as the radius is to the sine of BAC.

Prop. 4.—If a plane surface moves with an equable velocity in a fluid, the height of a column of that fluid which would generate the velocity of the plane, in a direction perpendicular to its surface is half the height of a column equal to the pressure on that surface.

For, the velocities of the forces of incidence and reflection being unaltered, the pressure in the direction of the resultant must be double to produce the same quantity of motion.

Prop. 5.—The height of a column of fluid equivalent to the direct resistance of a plane surface moving in it, varies directly as the square of the velocity and as the cube of the sine of the plane's inclination to the direction of the motion; or as $\frac{v^2 \sin^3 a}{32\, r^3}$.

Let v be the velocity of the plane, a the angle it makes with the direction of the motion, and h the height of the column equivalent to the pressure perpendicular to the plane.

Then (Prop. 3.) $r : \sin a :: v : \frac{v \sin a}{r}$ = the velocity in a direction perpendicular to the surface; and since (by Prop. 4.) $\frac{v \sin a}{r} = \sqrt{\frac{64\,h}{2}}$; we have $\frac{v^2 \sin^2 a}{32\,r^2} = h$.
But the resistance in a direction perpendicular to the surface is to the resistance in the direction of the motion as rad : $\sin a$; (Philosophical Magazine, vol. lxviii. p. 13.) hence $r : \sin a :: \frac{v^2 \sin^2 a}{32\,r^2} : \frac{v^2 \sin^3 a}{32\,r^3}$ = the height of a column of the fluid equivalent to the resistance.

Cor. The weight of the column will be $\frac{c\,w\,v^2 \sin^3 a}{32\,r^3}$, when c is the area of the base, and w the weight of a cubic foot of the fluid.

Prop. 6.—The direct resistance of a solid having a circular base is $\int \frac{p\,v^2 w\,y\,\dot{y} \sin^3 a}{16\,r^3}$.

Let $p = 3{\cdot}1416$, and y = the variable radius of the base; then $c = 2\,p\,y\,\dot{y}$; hence $\int \frac{p\,w\,v^2\,y\,\dot{y} \sin^3 a}{16\,r^3}$ = the fluent of the resistance.

1. When the solid is a cylinder the plane surface of the end being perpendicular to the direction of the motion $\frac{p\,w\,v^2\,r^2}{32}$ = the resistance.

2. If the solid terminate in a cone of which the slant side is r, and the $\sin a = y$, we have $\frac{p\,w\,v^2\,y^5}{32\,r^3}$ = the resistance. When $r^2 = 2\,y^2$; we have $\frac{p\,w\,v^2 y^2}{1{\cdot}68 \times 32}$ = the resistance, whence the direct resistance of a cylinder is to such a cone as 168 : 100.

3. When the solid terminates in a hemisphere, $\overline{r - x} = \sin a$, and $y\,\dot{y} = \overline{r - x}\,\dot{x}$; therefore $\int \frac{p\,w\,v^2 (r-x)^4 \dot{x}}{16\,r^3} = \frac{p\,w\,v^2\,r^2}{5 \times 16}$ = the resistance: hence, the resistance of a hemisphere is to that of a cylinder as 2 : 5 or as 10 : 25.

Leaving these cases I will now return to the effect of the fluid closing upon the body behind it.

The velocity with which the fluid closes on the body it is obvious should be estimated in a direction perpendicular to the surfaces, and, following the same train of reasoning as before, we shall find no difficulty in determining its effects.

Prop.

PROP. 7.—The pressure of a fluid in a direction perpendicular to the surface which it follows, is as the difference between the height of a column equal to the pressure of the fluid, and the height of a column of fluid capable of generating the velocity of the surface.

Let m be the height of a column equal to the pressure of the fluid on a surface at rest, and $h =$ the height of the column capable of generating the velocity; then $m - h =$ the height of the column which generates the effective motion, for whatever pressure is lost by the motion of the surface must be subtracted.

PROP. 8.—The impelling force of the fluid which follows a body in motion, in the direction of the motion, is equal to a column of fluid whose height is $\frac{64\,m\,r^2 \sin a - v^2 \sin^3 a}{64\,r^3}$.

Let v be the velocity of the body, then the velocity with which the fluid flows in a direction perpendicular to the surface is $\frac{v \sin a}{r}$, (Prop. 3.) and the head h equivalent to this velocity is $\frac{v^2 \sin^2 a}{64\,r^2}$; consequently, (Prop. 7) $m - h = m - \frac{v^2 \sin^2 a}{64\,r^2} = \frac{64\,m\,r^2 - v^2 \sin^2 a}{64\,r^2}$; and, this pressure reduced to the direction of the motion is $\frac{64\,m\,r^2 \sin a - v^2 \sin^3 a}{64\,r^3} =$ the effect of the fluid which follows the body.

Cor. The anterior and posterior forms of the body should not be similar figures in the solid of least resistance.

PROP. 9.—The total resistance of a fluid to a body moving in it is equal to the pressure of a column of the fluid of which the height is $\frac{3\,v^2 \sin^3 a - 64\,m\,r^2 \sin a}{64\,r^3}$, when the ends are identical figures.

We have found the direct resistance to be $\frac{v^2 \sin^3 a}{32\,r^3} = \frac{2 v^2 \sin^3 a}{64\,r^3}$, from whence if we deduct the effect of the fluid which follows it, we have $\frac{2 v^2 \sin^3 a - 64\,m r^2 \sin a + v^2 \sin^3 a}{64\,r^3}$; and $\sin a$ in both parts of the expression being made of the same value from identity of figure, we have $\frac{3\,v^2 \sin^3 a - 64\,m\,r^2 \sin a}{64\,r^3} =$ the column equivalent to the whole effect.

Hence we see that the resistance is not as the square of the velocity; and besides, this equation shows the important circumstance, that when the velocity exceeds that which the medium is capable of propagating, the fluid must accumulate before the body, till its density, or modulus of elasticity, becomes sufficient

sufficient to communicate motion to the distant parts of the fluid in the direction of the motion of the body. That such increase of resistance as this formula indicates, takes place in great velocities has been ascertained by experiments on the motion of cannon balls.—I hope this paper contains, for those who are acquainted with the subject in theory and experience, sufficient evidence of the accuracy of my views; and if I have any where trodden in the steps of others it is not knowingly, and there is strong presumption that I have not done so, in the fact that I have not the slightest recollection of any theory founded on the same principles.

August 1826. THOMAS TREDGOLD.

XVI. *Properties of the Trapezium.* *By* T. S. DAVIES, *Esq. of Bath**.

AT a very early period of my mathematical studies the trapezium seemed to offer a far more open field of inquiry, and to yield far more interesting results, than any other figure, the circle excepted, that I examined. The properties of the triangle, numerous as they were, had been so often examined, and their characters stated under almost every possible variety of aspect, that little really novel appeared likely to result from any inquiries, however well directed or vigorously pursued. The trapezium on the contrary had been investigated very little further than its elementary and more obvious properties; and the majority even of these were such as either had some analogy to the triangle, or resulted from dividing it into subordinate triangles. The ground has been occasionally trodden by others; but I think it will be found that every one who has done so has made his discoveries more by accident, or to meet some mathematical exigency, than from any systematic plan of inquiry: and the best proof of this is, that amongst the great men who have noticed any of the properties of the trapezium, not one of them has cared to pursue those results into any of their remoter consequences.

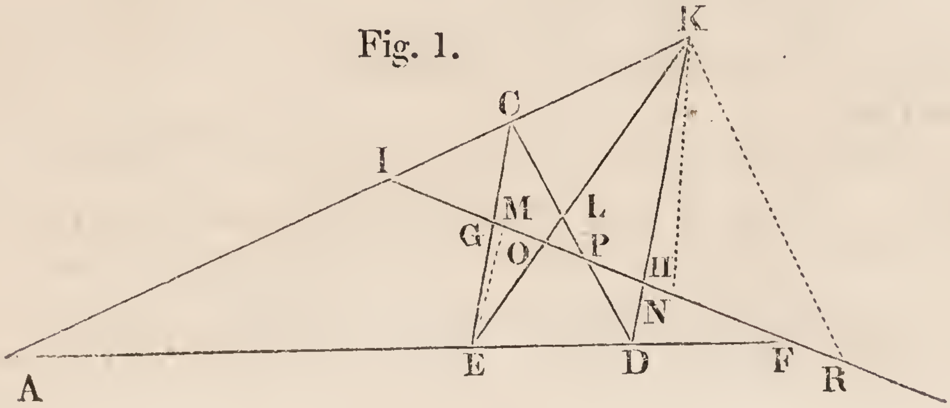
In the course of my researches I have been so fortunate as to meet with several classes of properties which, whether viewed as so many elegant theorems, or in reference to their application to subsequent inquiries, cannot fail to interest the geometrical student; and shall do myself the honour to present to this Society a series of short papers containing a few of the properties that I have noticed, together with a general outline of their demonstrations.

* Read before the Society of Inquirers, Bristol, August 5, 1825, and communicated by the Author.

ECKD

ECKD a trapezium,—whose opposite sides meet in A and B and whose diagonals intersect each other in L,—is cut by the transversal I, F as in the figure.

Fig. 1.



Prop. I.

$$FD\,.\,CI : FE\,.\,KI :: CG\,.\,DH : GE\,.\,KH\ *.$$

Dem.—Draw ME parallel to KD, and KN to CE. Then

$$EG : EM :: KN : KH,$$
$$KN : CG :: IK : IC,$$
$$EM : DH :: EF : DF;$$ and compounding

$$FD\,.\,CI : FE\,.\,KI :: CG\,.\,DH : GE\,.\,KH.$$ Q. E. D.

Cor. 1. When F and I coincide with A, we have $DA\,.\,AC : EA\,.\,EA\,.\,AK :: CG\,.\,DH : GE\,.\,KH$, and $\therefore CG\,.\,DH : GE\,.\,KH$ a constant ratio when the transversal passes through A.

Cor. 2. When $CG\,.\,DH : GE\,.\,KH :: DA\,.\,AC : GE\,.\,KH$ the transversal passes through A.

Cor. 3. When the transversal passes through A and B

$$CA\,.\,AD : KA\,.\,AE :: CB\,.\,BD : EB\,.\,BK.$$

Prop. II. (Fig. 1.)

$$IC\,.\,FE : IK\,.\,FD :: EO\,.\,PC : OK\,.\,PD.$$

Dem.—Draw ME parallel to KD, and KN to CE. Then

$$FE : FD :: EM : DP,$$
$$EM : KN :: OE : OK,$$
$$IC : IK :: PC : KN;$$ hence, compounding

$$IC\,.\,FE : IK\,.\,FD :: EO\,.\,PC : OK\,.\,PD.$$ Q. E. D.

* To meet all the varieties which result from taking the trapezium successively *salient*, *re-entrant*, and *intersectant*, and from interchanging C and K in each of these classes, would require upwards of forty figures; but it is unnecessary to insert them here, as the geometrical reader will easily sketch them for his own use. *Forty-two* is the number I have drawn, but I cannot positively affirm that all possible cases are included in this list.

Cor.

Cor. 1. When F and I coincide with A, we learn that

CA . AE : KA . AD : : CP . EO : OK . PD.

Cor. 2. When O and P coincide with L, we have

IC . FE : IK . FD : : CL . LE : KL . LD.

Cor. 3. When the transversal passes through A and L, we find that

CA . AE : KA . AD : : CL . LE : KL . LD.

Cor. 4. When F and I coincide with A, we have

CL . LE : KL . LD : : CP . EO : OK . PD.

Cor. 5. Similarly we find when O, P, L coincide

IC . FE : IK . FD : : CA . AE : KA . AD.

Prop. III. (Fig. 1.)

GC . HK : GE . HD : : KO . CP : OE . PD.

Dem.—Draw EM, KN as before, and KR parallel to CD. Then

CG : KN : : CP : KR,
KN : GE : : KO : OE,
KH : K^3R : : H^2D : PD; hence

CG . KH : GE . HD : : KO . CP : OE . PD. Q. E. D.

Cor. 1. When the transversal passes through L, we find

CG . KH : GE . HD : : CL . LK : EL . LD.
: : CB . BK : EB . BD.

Prop. IV. (Fig. 1.)

Let the points ECKD lie in the circumference of a circle; then

AE : AK : : CL : LK : : EL : LD : : AC : CD.

Dem.—By circle AE . AD : AC . AK : : CL . LD : EL . LK, and (Cor. 1. Pr. 2.) AE . AC : AD . AK : : CL . LE : KL . LD; hence, &c. Q. E. D.

Prop. V. (Fig. 1.)

The points ECKD being still in the circle we shall have

KC . CE : KD . DE : : CL : LD, and
KC . KD : CE . ED : : KL : LE.

Dem.—The triangles CLE, KLD are similar, and

CE : KD : : EL : LD
CK : ED : : CL : LE; hence compounding
KC . CE : KD . DE : : CL : LD.

In like manner is the second analogy demonstrated. Q. E. D.

Cor. Hence also $CE^2 : KD^2$: : CL . LE : KL . LD,
: : CA . AE : KA . AD,
and $CK^2 : ED^2$: : CL . LK : EL . LD
: : BC . BK : EB . BD.

Prop.

Prop. VI. (Figs. 2 and 3.)

Fig. 2.

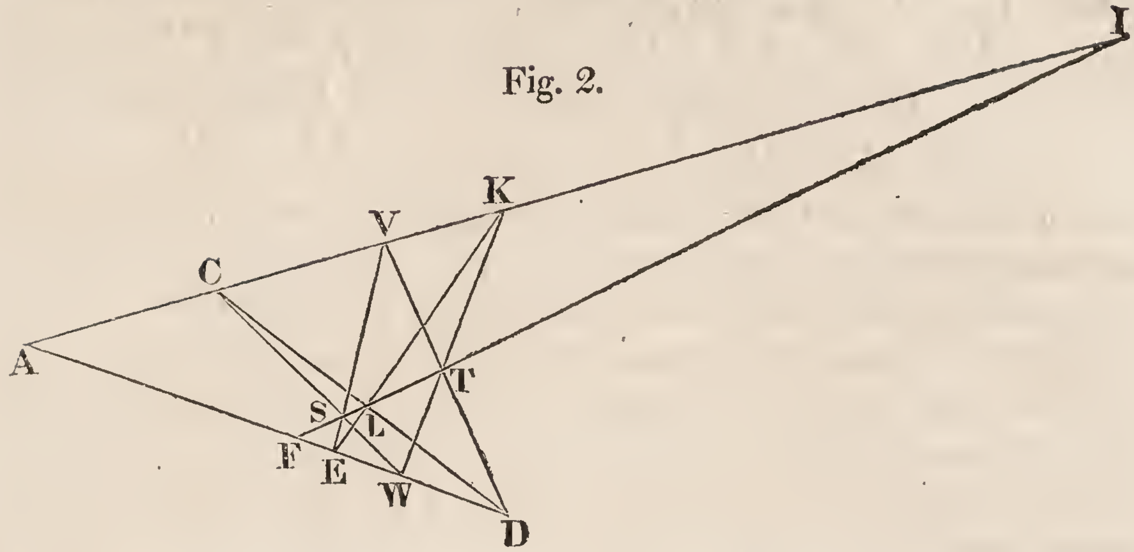
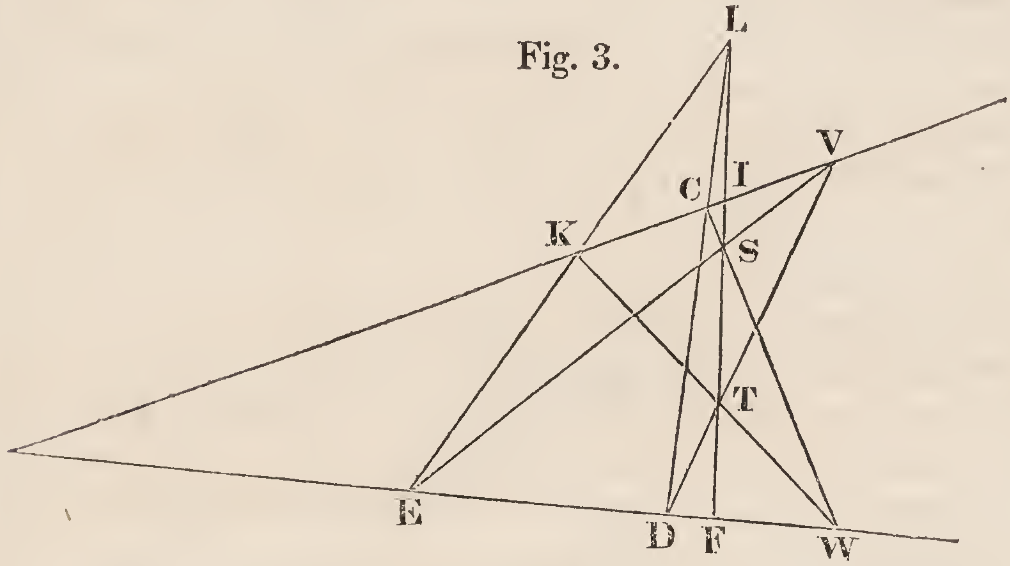


Fig. 3.



Upon the opposite sides ED, CK of a trapezium are constituted the triangles EVD, CWK, each having its vertex in the other's base. If S be the point of intersection of CW, EV, and T that of VD, WK; then will ST always pass through L, the intersection of the diagonals CD, KE of the trapezium*.

Dem.—Let A be the intersection of KC, ED, and ST cut CK, ED in I and F. Then

$$CS \,.\, SE : VS \,.\, SW :: CA \,.\, AE : AV \,.\, AW,$$

$$VT \,.\, TW : KT \,.\, TD :: AV \,.\, AW : KA \,.\, AD \; (ib.); \text{ hence}$$

$$\frac{CS \,.\, SE}{VS \,.\, SW} : \frac{KT \,.\, TD}{VT \,.\, TW} :: CA \,.\, AE : KA \,.\, AD$$

$$CL \,.\, LE : KL \,.\, LD \;.\;.\;.\;.\; (a)$$

Again, $IC \,.\, FE : IV \,.\, FW :: SC \,.\, SE : VS \,.\, SW \; (ib.)$

$$IV \,.\, FW : IK \,.\, FD :: VT \,.\, TW : KT \,.\, TD \, (ib.) \text{ hence}$$

$$\frac{CS \,.\, SE}{VS \,.\, SW} : \frac{KT \,.\, TD}{VT \,.\, TW} :: IC \,.\, FE : IK \,.\, FD \;.\;.\;.\;.\; (b)$$

* This theorem also includes a considerable number of figures, which however it is unnecessary to insert here.

Hence

Hence from (*a*) and (*b*) we find

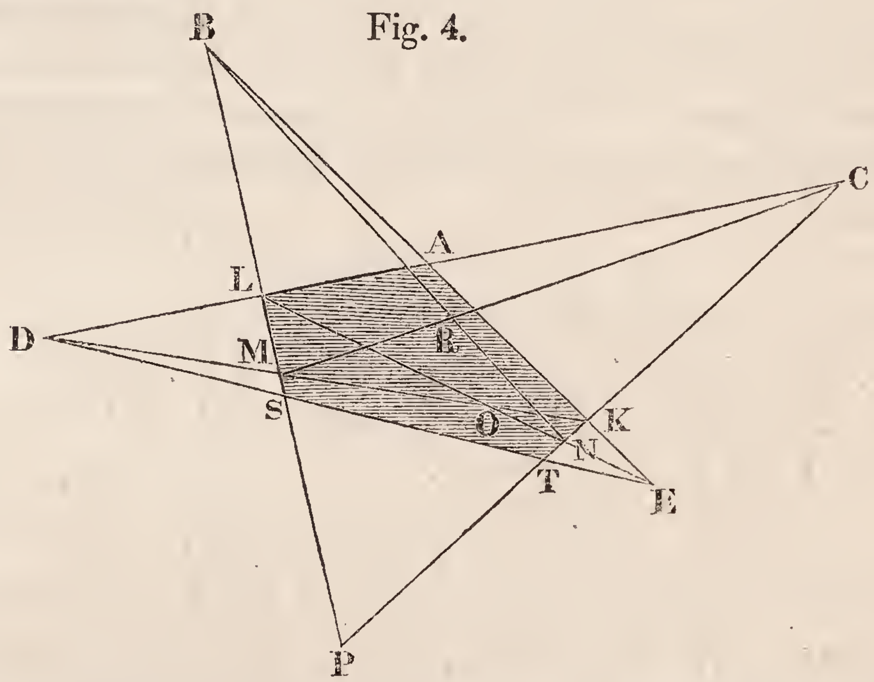
IC . FE : IK . FD : : CL . LE : KL . LD; and thence (Converse of cor. 2. Pr. 3.) that IF or ST passes through L.

Q. E. D.

The geometrical reader will readily perceive that C and K may interchange their places, so as to throw L without the trapezium whilst a similar mode of proof obtains. This will be rendered obvious from constructing the figure.

Schol. From this curious property a great number of beautiful porisms may be deduced; and it is probable that very many *linear loci* may be brought within the reach of actual demonstration by the efficiency of this single theorem. I gave a demonstration of it from other principles in the Monthly Magazine for July 1825. This property is not, however, yet stated in its most general or its most interesting form; for instead of being posited in the sides of the angle KAD, the points V and W may range *in the periphery of any Conic section whatever*, the points ECKD being in the same periphery. The demonstration depends upon a system of investigation something different from that employed in this paper: I shall therefore defer the proof till I have completed a paper upon which I am now employed, in which I trace a number of related properties of that class of curves. I shall, however, here set down two or three corollaries from Pr. 6.; for such they may be called, as they flow from it without the slightest reducing analysis.

Fig. 4.



Cor. 1. Let BDPEC (fig. 4) be a *pentalpha**, formed by pro-

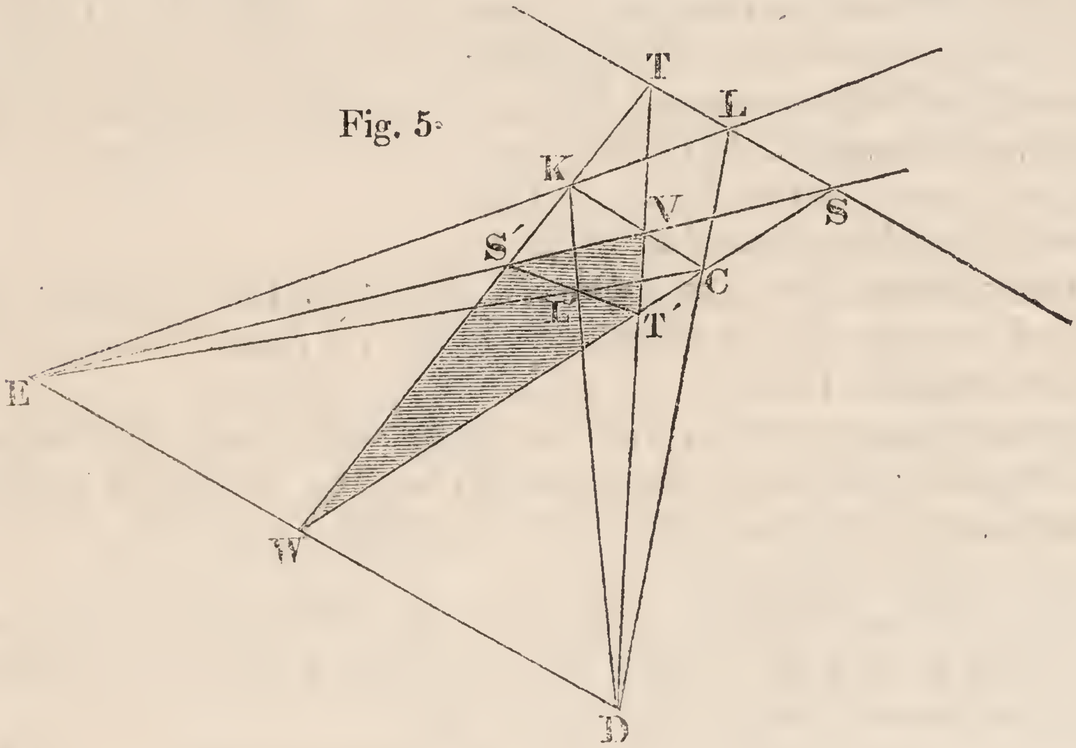
* This very appropriate name was given to the figure, I believe, by J. C. Hobhouse, Esq. M.P.; at least I do not recollect to have met with it except in his "Illustrations of the Fourth Canto of Childe Harold." This figure,

prolonging the sides of the pentagon ALSTK till they meet. Draw DK cutting EL in O and BP in M, and draw EL cutting PC in N. Then if R be the intersection of NB, MC, the line OR will tend to A, the summit of the pentagon.

Cor. 2. BE, CD (fig. 4) are two straight lines intersecting in A; let two points B, E and C, D be taken one pair in each of these lines, and P a point between those lines. From P draw PB, PC cutting DC in L and BE in K; draw DK cutting BP in M and LE in O, and let LE cut PC in N. Lastly, draw NB, MC intersecting each other in R. Then OR always tends to the same point, the intersection of the lines BE, CD.

This corollary has fifteen separate cases, all of which are referable to the different cases of Prop. 6.

Fig. 5.



Cor. 3. Let S′VT′W (fig. 5) be any trapezium whatever. Through

figure, however, had been long celebrated amongst the Greeks, Romans, Etruscans, and other nations, before it was introduced into this country. Whether it was a Druidical figure I have not been able to learn, though I should think it highly probable that it was. The *medial section*, or *extreme and mean ratio*, probably obtained the name of the "*divine section*," from its necessity in constructing the pentagonal base of this figure, and the figure itself having been considered emblematical of the Deity.

Bishop Wilkins, speaking concerning characters that express words, goes on to say: "Of this nature was that angular figure so much used by the Grecians of old, which might be resolved into the letters υγιεα.

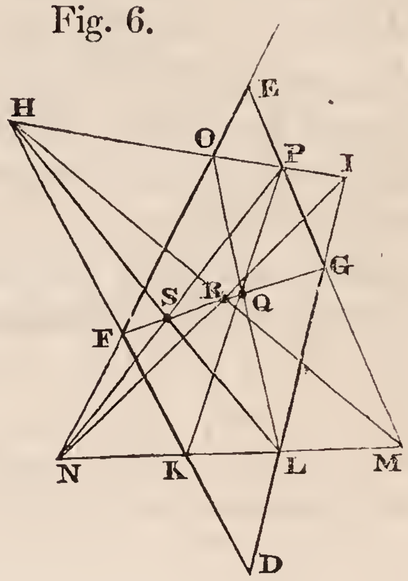
"This mark was esteemed so sacred amongst the ancients, that Antiochus Soter, a perpetual conqueror, did always instamp it on his coin, and in-

 scribe

Through either pair of opposite angles, as V and W, draw any lines KC, ED limited by the sides. The lines DK, EC will intersect in the diagonal S′T′, and the lines EK, DC will intersect in the diagonal ST of the intersecting trapezium TS′ST′.

Prop. VII.

Fig. 6.



Let EFDG be any trapezium whatever, and through any point R in either of the diagonals, as in FG, draw lines, each to cut an opposite pair of sides in H, M and N, I; join also HI cutting the pair of sides EF, EG in O and P and NM cutting the pair FD, DG in K and L. Then OL, PK being drawn, their intersection Q is in the diagonal FG.

Dem.—Draw PN, HL intersecting in S; then since HLI, NPM are two triangles situated as in Pr. 6, the line joining their intersections S, G will pass through R the intersection of the diagonals HM, NI; and F, S, R, G, are in a straight line.

Again, because NOL, HKP, are triangles also situated as in Prop. 6. and intersecting in F, Q, the line FQ passes through S the intersection of the diagonals HL, NP; or Q is in the line FG. Q. E. D.

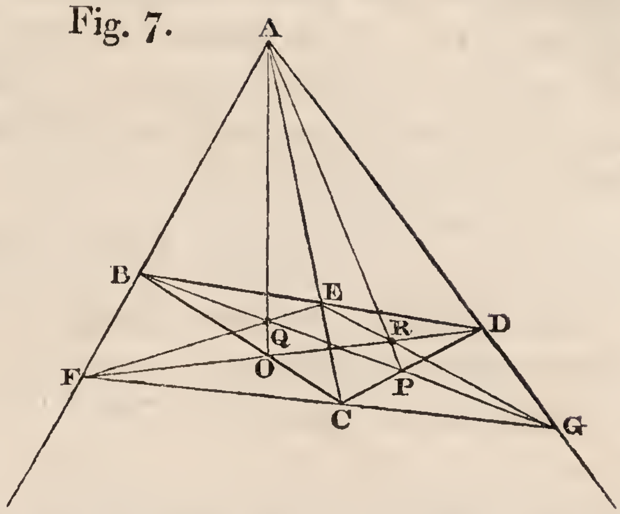
Cor. When HI passes through E, the points O, P coalesce with E and K, L with D; in which case, the theorem becomes the same with Cor. 3. Pr. 6.

scribe it on his ensigns; unto which he did pretend to be admonished in a dream by an apparition of Alexander the Great. And there are many superstitious women in these times who believe this to be so lucky a character, that they always work it upon the swaddling clothes of their young children, thinking thereby to make them healthful and prosperous in their lives. Unto this kind also, some refer the characters that are used in magic, which are maintained to have not only a secret signification, but likewise a natural efficacy."—Works, vol. ii. p. 49.

Mr. Hobhouse thinks the figure found its way into the Northern superstitions from its similarity to the hammer of Thor. He remarks that "the English shepherd who never heard of Antiochus, nor saw his coin, still cuts it in the grass": and perhaps some analogy might be traced by those who are curious in matters of this kind, between the signification of the heraldic mullet, the cinquefoil, and the rose, when compared with the signification of the figure in the older superstitions.

Prop. VIII.

Fig. 7.



Through either angle as C of any quadrilateral figure ABCD draw a line FG limited by the other sides AB, AD in F and G. Join GB, FD cutting the sides BC, CD in O and P, and let AO, AP cut BG, FD in Q and R. Then FQ and GR will pass through E, the intersection of the diagonals AC, BD.

Dem.—The triangles FCA, PBD being constituted in the trapezium FPDA according to the conditions of Pr. 6. show that E, R, G, are three points in the same straight line. Also, the triangles GCA, BDO in the trapezium GOBA, show in like manner that E, Q, F, are points in the same straight line.

Q. E. D.

Prop. IX.*

Fig. 8.

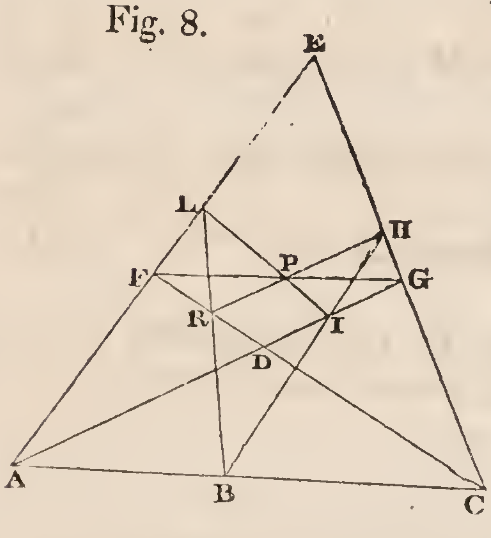
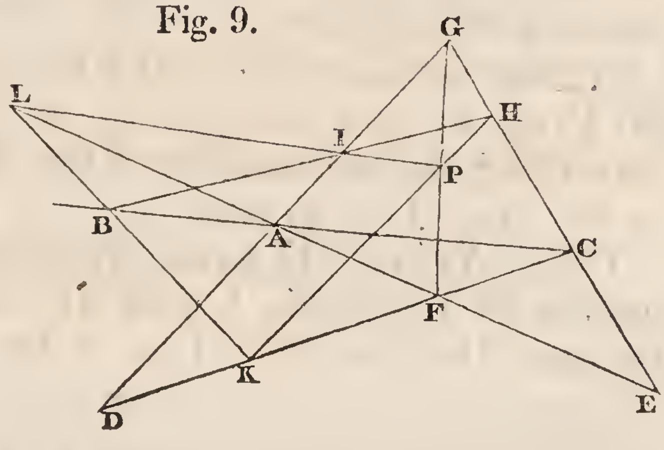


Fig. 9.



Let EFDG be a trapezium, whose opposite pairs of sides EF, GD and EG, FD*, meet in A and C. From any point B in the line AC, draw lines to cut the adjacent pairs of sides of the trapezium, viz. BIH, to cut DG in I and EG in H, and BKL to cut FD in K and FE in L. Then LI, KH will always intersect in the diagonal FG.

Dem.—1. The transversal AC cutting the triangles KEL and IGH give

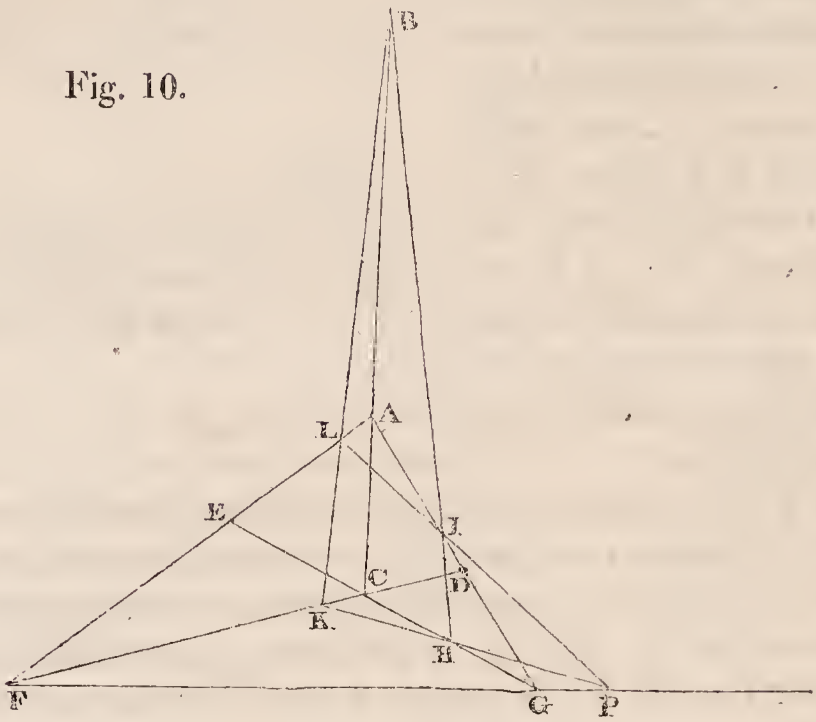
BK : BL : : AF . CK : AL . FC, (Bland's Geom. Prob. Pr. 47. Sect. 4.) and

BI : BH : : CG . AK : CH . AG. Hence, compounding

* It is to be taken for granted, that all lines given by position may be prolonged when necessary, as well as that lines joining given points, are also drawn.

$BK . BI : BL . BH :: AF . CK . CG . AK : AL . CF . CH . GA.$

Fig. 10.



2. Again, the triangles ALI, CKH, cut by the tranversal FG in the points P' and P'', give

$$LP' : P'I :: GA . FL : IG . AF \text{ (Bland. ib.) and}$$
$$HP'' : P''K :: FC . GH : KF . CG. \text{ Whence, comp.}$$

$LP' . P''H : P'I . P''K :: GA . FL . FC . GH : IG . AF . KF . CG.$

3. Further, the triangles AFC, ACG give

$$LF : LA :: BC . KF : AB . CK. \text{ (Bl. ib.) and}$$
$$GH : CH :: GI . AB : BC . IA. \text{ (ib.) Hence}$$

$\frac{LF . GH}{CH . AL} = \frac{IG . KF}{CK . AI}.$

4. Dividing the terms of the first proportion by the corresponding terms of the second, we have

$\frac{LP' . HP''}{LB . HB} : \frac{P'I . P''K}{BI . BK} :: \frac{FL . GH}{CH . AL} : \frac{IG . KF}{CK . AI}.$ But by (3) we learn that the second of these is a ratio of equality; hence

$$\frac{LP' . HP''}{LB . HB} = \frac{PI . P''K}{BI . BK}, \text{ or}$$

$$LP' . HP'' : P'I . P''K :: LB . BH : KB . BI,$$
$$:: LP . PH : IP . PK \text{ (Cor. 3. Pr. 2),}$$

and hence (converse of Cor. 2. Pr. 3.) the lines LI, KH, and FG intersect in the same point P. Q. E. D.

Cor. 1. Conversely we find, *ex absurdo*, that when LI, KH, situated as in the theorem, intersect in FG, the lines LK, HI, will intersect in AC.

Schol. The three figures above given are sufficient for the purpose

purpose of showing the nature of the mutations which the theorem undergoes when we take the trapezium successively *salient* (fig. 8), *re-entrant* (fig. 9), and *intersectant* (fig. 10). The number of figures, however, which a complete exhibition of all the cases of the theorem would require, is not less than a hundred and twenty; perhaps even more, which have escaped my notice. These three figures are sufficient to show, also, that had we defined AC, FG, DE, the three diagonals of the trapezium in fig. 8, an elegant enunciation might be formed for the combination of them in pairs in reference to this theorem.

It is easy by a similar train of investigation to discover several analogous and equally beautiful properties of the trapezium: but as this paper has already exceeded the limits which I originally proposed to myself, I shall defer them to a future but not distant period. In conclusion, I shall just remark, that from the properties already given a series of interesting properties of the conic-sections are immediately deducible.

August 5, 1825.

XVII. Decas sexta novarum Plantarum Succulentarum; *Autore* A. H. Haworth, *Soc. Linn. Lond.—Soc. Horticult. Lond.—necnon Soc. Cæs. Nat. Curios. Mosc. Socio, &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

HAVING just finished my sixth Decade of new Succulent Plants, I lose no time in sending it for insertion in the next Number of your Magazine.

All the plants included in this decade, appear to me, after the most diligent research, to be entirely nondescript, and belong to the vast genus *Mesembryanthemum*; to which they form a conspicuous addition: for some of them are not only new as *species*, but constitute *new* and unrecorded *sections*. They are all flourishing and flowering in the royal and unrivalled gardens at Kew; and were sent thither by our most assiduous friend Bowie, whose adventurous and discriminating eye detected them growing spontaneously in the remote and arid regions of Southern Africa, whither this enterprising traveller is again about to repair. And he proposes to go not only with the intention of collecting plants, seeds and roots, but also, as heretofore, in almost all the branches of animated nature.

And now, sir, most heartily wishing this extensive discoverer of succulent plants every possible success in his laborious undertakings,—

undertakings,—a pleasant voyage to the Cape of Good Hope, a prosperous and excursive sojourn there,—a safe return to his native home, and a profitable termination to his interesting speculations; I remain, very respectfully,

your old correspondent and friend,

Chelsea, August 1826. A. H. Haworth.

Classis et Ordo. Icosandria Pentagynia.

Genus, Mesembryanthemum *Auctorum*.

Sectio (nova) Albinota, subacaulia: cæspitosa: radice perenni, foliis decussatis integris obliquè incurvis viridibus patulis crebrè albo tuberculatim magnipunctatis, infernè basive semiteretibus, supernè acinaciformiter triquetris vel ferè æquilateralibus, plùs minùs mucronulatis; floribus centralibus solitariis sessilibus luteis, staminibus erecto-patentibus ut in sectione cui nomen Ringentia, anteque eam locanda.

albinotum. M. (keeled, white-dotted) foliis acinaciformiter sur-
1. sùm triquetris mucrone recurvulo punctisque subelevatis albicantibus sparsis.

Habitat C. B. S.

Florebat in regio horto Kewense, Sept. 1825. G. H. ♃. Anno priore ortum ex seminibus africanis a Domine Bowie collectis. Sequentia omnia quoque ejus assiduitati debemus.

Obs. Facies ferè *M. aloöidis* Nob. (absque ejus collectis staminibus).

Folia sub-biuncialia, latitudine 5-lineari, albicantibus punctis crebris quasi macularibus, et (certo situ) subindè sæpè squamulas pallidas simulantibus, rariùs viridibus. *Flos* unus solùm vidi mediocris luteus ferè ut in *M. felinum* s. *M. lupinum* Nob. at A. M. expansus, (nec vespere,) basi *bractæis* duabus magnis foliiformibus. *Calyx* 5-fidus, laciniis subæqualibus harum tribus obtusissimis aliquot brevioribus: duabus longè acutioribus parùmque longioribus. *Corolla* subsesquiuncialis, *petalis* latis apice crenulatis denticulatisve. *Stamina* brevissima lutea, *antheris* polliniferis saturatioribus cum luteo. Cætera non examinavi.

albipunctum. M. (small-keeled white-dotted) foliis semitere-
2. tibus albo tuberculatim punctatis, supernè triquetris, mucronulo rufescente.

Habitat C. B. S.

Florebat

Florebat in regio horto Kewense ad finem Sept. 1825. G. H. ♃.

Obs. Priori simile, at foliis duplò triplòve minoribus numerosioribus, a basi ad apicem sæpiùs attenuatis nec auctim carinatis seu subacinaciformibus ut in priore, punctis magìs albis, sed flore (unico qui solùm vidi et non dissecavi) ut in illo.

β. *majus*, fere duplò.

Sectio, Ringentia *Nob.* in Philos. Mag. Aug. 1824.

Ermininum. 3. M. (The Ermine Chop) subacaule: glaucum: foliis rugoso-magnipunctatis: marginibus supernè breviden-tatis, carinâ integrâ.

Habitat C. B. S.

Florebat in regio horto Kewense, Maio 1825. G. H. ♃.

Descriptio. *Herba* densè cæspitosa. *Folia* subinde integra, at sæpiùs marginibus apicem versus 4—6-dentatis, dentibus validiusculis patentibus acutis, at sine setulâ finientibus. *Flores* solitarii terminales subsessiles absque bracteis. *Calyx* 5-fidus, infernè (cum germine) compresso-turbiniformis, supernè foliolis oblongis æqualibus apice obtusis et obtusè subcarinatis, sed obsoletè et plùs minùs membranaceis, grossè tuberculato-punctatis foliorum more, at cum punctis oblongioribus. *Corolla* subuncialis ferè ut in *M. mustelino*, vespere aperta, usque ad decimam horam (Augusto mense) petalis numerosis variè laxèque expansis capillaribus luteis calycinis foliolis 2 lineas superantibus. *Filamenta* longitudine valdè variantia, *petalis* semper duplò breviora lutea *antheris* longis cum polline subaurantiaco, infernè cum *calyce* altè coalita. *Styli stigmatave* 5 semilineam longi obtusi (lente intùs ramentacei) basi coaliti in unum *stylum* brevissimum subconicum pulposum. *Germen* incipiens depressum. Unum florem mense Septembre, vespere suaveolentem (at minùs gratum quàm *M. mustelinum*) solùm vidi.

β. *majus* corollâ luteâ apice rubro.

Obs. Ante *M. murinum* cui simile, certè locandum, et distinguitur foliorum crassiorum rugosiorum carinis integris, marginalibus dentibus brevioribus, absentiâque setulæ terminalis.

agninum. 4. M. (The Lamb's Chop) subacaule: canescens: punctato-rugosulum: foliis semiteretibus puncto-serrulatis, subinde dentatis; basi intùs pustulatis.

Habitat

Habitat C. B. S.

Vigebat in regio horto Kewense, 1824, et florebat Maio 1826. G. H. ♃.

Descriptio. Inter ejus affinia herba conspicua cæspitosa crassa et forsàn suæ sectionis. *M. albidi* habitus at 4-plò minùs, undique asperulum et ferè rugosulum punctis elevatis numerosissimis. *Folia* sub-biuncialia, semunciam circiter lata decussata erecto-expansa, apice angustiora, at obtusa (junioribus obsoleto mucronulo) marginibus paulò post medium sub obsoletè obtuséque repando-dentatis, basi internè pustulâ magnâ albâ, et foliorum modo punctatâ. *Florem* unum sessilem, centralem, ebracteatum luteum perientem, ferè ut in *M. mustelino*, at longè majorem, (sed non magnum) solùm vidi. Inde locum speciei hujus apparet, et prope *M. mustelinum* locarem.

β. Paulò minus, foliorum dentibus obscurioribus.

γ. foliis erectioribus integris.

Sectio, Adunca *Nob.* suffrutices vix semipedales, foliis subulatis teretiusculis sæpè incurvo-recurvulis, apicibus semper attenuatis aduncis; floribus rubicundis antemeridianis, sæpiùs autumnalibus.

inconspicuum. M. (small-flowered, rigid) prærigidum: foliis
5. trigono-semiteretibus parvis aduncis, floribus solitariis minutis terminalibus.

Habitat C. B. S.

Florebat in regio horto Kewense A.D. 1825, autumno, rursúmque Junio, &c. 1826. G. H. ♃.

Descriptio. Suffrutex semipedalis dumosus, ramis patulis adscendentibus incurvisve filiformibus duris, junioribus lentis ope furvo-puberulis, apice ad solem coruscantibus. *Folia* distincta semuncialia gracilia incurvata, et ad solem aëre aperto subpapuloso-micantia viridia, vix glaucescentia, apicibus attenuatis aduncis rufis, sive morientibus fulvis. *Flores* inconspicui subtrilineares ebracteati, pedunculis brevissimis. *Calyx* 4-fidus ordinarius. *Corolla* antemeridiana saturatissimè rubicunda, *petalis* (ratione magnitudinis) latis obtusis, et ferè semper integris. *Filamenta* collecta, erecta, apice supernéve saturatiora colore, *antheris* (ante anthesin) albis.

Florem unum solùm examinavi, sed alios varios vidi.

Sectio (nova) Crocea. Suffrutices ramulis secundis, foliis crassis glauco-cærulescentibus basi semiteretibus

teretibus, supernè obsoletè triquetris, floribus terminalibus solitariis minoribus vel mediocribus inodoris, primò sæpè luteis, denique sæpiùs croceis; pedunculis, calyceque inæquali, succulentis. Sectio, a sectione SEBACEA separanda, quæ ultima gaudet ramulis decussatis, foliis teretiusculis crassioribus cærulescenti-glaucis, floribus umbellatis parvis suaveolentibus flavis mellinis.

Obs. Sectioni CROCEA referenda sunt *M. croceum* Jacq.—*M. purpuro-croceum* Nob. et ejus var. *flavo-croceum* (quæ *var.* nunc veram speciem propono)—atque etiam duæ sequentes, viz.

luteum. M. (upright yellow) foliis obtusis, floribus minoribus;
6. caule ramuloso erecto rigido, nodis tumentibus radiculigeris.

Habitat C. B. S.

Florebat in regio horto Kewense, Junio 1823, et per æstatem. G. H. ♄.

Descriptio. M. flavo-croceo supradicto certè proximum, at elatius, ramosius, sed longè gracilius, ramis duris, foliis floribusque 3-plò minoribus.

In tertio anno erectum pedale ultráque est, ramorum nodis tumentibus terram versus (ope lentis) pullulantibus radiculis farctis. *Folia* erecta inter nodiis longiora lævia cærulescenti-glauca. *Flores* pauci solitarii, in summis ramorum altiorum terminales diurni lutei, *pedunculis* ebracteatis clavatis succulentis. *Calyx* 4-phyllus, foliolis præinæqualibus ut in *M. purpuro-croceo*, duobus parvis membranatis: et 2, 4-plò majoribus emembranatis. *Corolla* lætè lutea, moriens saturatior. *Filamenta* erecta brevia *antheræ*que luteæ. *Styli* 8? minuti erecti, filamentis multoties breviores, pallidiores ovatò-acuminati, per lentem ramentacei. *Germen* parvum pulposum 8-loculare.

luteolum. M. (acute-leaved, small yellow) foliis confertioribus
7. apice acuto recurvulo; ramis gracilibus densioribus, floribus parvis diurnis. *M. læve* Th. Prod.?—Certissimè non Aitoni.

Habitat. C. B. S. . . . . G. H. ♄.

Cum priore florebat in regio horto Kewense at quadruplò humilius, ramis densioribus, longéque gracilioribus, nodis minùs tumentibus absque radiculis pullulantibus, et subindè flexuosè decumbentibus. *Folia* paulò minora, confertiora, glauco-cærulea, at colore viridi pallidiore.

diore. *Flores* numerosiores. In cæteris priore quadrat omninò.

Obs. ab ultimo optimè distinguitur, foliorum apice acuto recurvulo, staturâque quadruplò minore.

Sectio Platyphylla, foliis ramulisve ferè semper plùs minùs ad solem papuloso-micantibus: radice subindè bienni annuâve.

clandestinum. 8. M. (minute flowering Myrtle-leaved) humifusum subpapulosum: foliis acutè ovatis petiolatis, petalis minutissimis.

Habitat C. B. S. . . . . . G. H. ♄.

Florebat per æstatem in regio horto Kewense A.D. 1824.

Obs. Vix *M. ovatum* Thunb. Prod. p. 88, propter minutissimos flores, sed in cæteris fortè concordat. Non *M. apetalum*, ejusdem auctoris in loco, propter folia non lineari-lanceolata. Neque *M. humifusum* Aitoni, quod folia spatulata produxit.

Planta nostra est herba valdè foliosa parva sive suffrutex, ramis numerosis herbaceis confertis procumbentibus 4-5 uncialibus teretibus, junioribus ad lucem papulosis, papulis (præcipuè foliorum) ad lentem elevatis numerosissimis distinctis convexis atque ad solem sublimpidis. Internodii foliis breviores. *Flores* sæpiùs terminales 1-3 nati pedunculo clavato tereti, folio breviore. *Calyx* 5-phyllus turbiniformis, foliolis subinæqualibus obtusis viridibus, apice (in aëre aperto) rubentibus, duobus minoribus basi submembranaceis ut in plurimis. *Corolla* omninò clandestina petalis numerosis linearibus obtusis albis, calyce humilioribus, et ferè invisibilibus; sed lentis ope corolla ad solem antemeridianum incurvo-erectula et aliquantillùm apertula est, ubi inclusæ *Antheræ* visæ sunt. *Stamina* brevissima alba, petalorum altitudine, *antheris* stramineis. *Styli* 5, minutissimi acuminati. *Capsula* subturbinato-globosa piso minor supernè 5-angularis, loculis totidem 5. *Flores* duo solùm examinavi.

Sectio Corallina. Suffrutices læves erecti lignosi; ramis concinnè decussatis numerosis cortice plùs minùs castaneo; foliis semicylindraceis valdè glaucis: floribus terminalibus, magnis mediocribusve, sæpissimè rubicundis.

lepidum. 9. M. (pretty, white-flowered) foliis apice recurvulis sub-

submucronatis: foliolis calycum acuminatis patulis, post anthesin marcescentibus.

Habitat C. B. S. . . . . . G. H. ♄.

Florebat in regio horto Kewense, Aug. 1825.

Obs. Suffrutex erectus gracilis, secundo anno subpedalis, *ramis ramulis*ve numerosis erectis. *Folia* ferè ut in *M. producto* Nob. (cui simillimum ut ovum ovo) at fortè minùs incurvula. *Flores* majores antemeridiani candidi formosi, pedunculis longioribus. *Styli* 5. *Capsula* supernè latior quàm in *M. producto*, 5-locularis minúsque umbilicata, loculis totidem 5. Cætera non examinavi.

Ab *M. producto*, cui proximum, calycum foliolis longè minoribus patentioribus perientibus, optimè distinguitur. Foliola calycina 2-3 in *M. producto* post florescentiam erecto-incurva carnosa auctaque persistunt.

Sectio Hispicaulia. Suffrutices dumosi subdodrantales caulibus pedunculisve plùs minùs hispidis: floribus antemeridianis rubicundis, rariúsve albis.

subcompressum. M. (slender upright twiggy) erecto-virga-
10. tum: foliis canescente-viridibus compresso-semiteretibus obliquè obtusissimis: ramis supremis parcè pilosulis.

Habitat C. B. S. . . . . . G. H. ♄.

Vigebat in regio horto Kewense A.D. 1825, et florebat perparcè in Julio 1826.

Descriptio. Suffrutex subrigidus in secundo anno subpedalis gracilioribus ramis majisque virgatis quàm in affinibus, summis solùm pilis parvis respicientibus, cæteris gradatim depilatis. *Folia* semper aliquantillùm compressa ad lucem canescente-papuloso-micantia, ferè ut in *M. candente* Nob. à quo dignoscitur erectis ramis foliorumque formâ. *Flores* terminales solitarii violaceo-rubicundi vivacissimi (nec albi ut in *M. candente*) *M. hispido* duplò minores longéque pallidiores. *Calyx* 5-6-phyllus, foliolis duobus foliiformibus majoribus obtusis, at nihilominùs valdè brevibus. *Petala* angusta. *Filamenta antheræ*que albæ, petalis triplò breviores. *Styli* 6, arcuatim radianter recurvuli subulati lutei, filamentis breviores.——N. B. Florum *pedunculi* perbreves, et cum *calyce* plùs minùs gradatim crystallino-pilosuli, ut in affinibus, at per scalam omninò minorem. *Flores* duo solùm examinavi.

β. *minus.* Varietatem ferè duplò minorem et ferè de-

pilatam ramis strictioribus vidi in ditissimo regio horto absque floribus. Fortè vera species. Simulat *M. brevifolium* parùm, at non effuse ramosum ut in illo, neque confertum.

XVIII. *An Account of some Geological Specimens, collected by Captain* P. P. King, *in his Survey of the Coasts of Australia, and by* Robert Brown, *Esq., on the Shores of the Gulf of Carpentaria, during the Voyage of Captain* Flinders. *By* William Henry Fitton, *M.D. F.R.S. V.P.G.S.*

[Continued from p. 34.]

VI. AS the superficial extent of Australia is more than three-fourths of that of Europe, and the interior may be regarded as unknown*, any theoretic inferences, from the slight geological information hitherto obtained respecting this great island, are very likely to be deceitful; but among the few facts already ascertained respecting the northern portion of it, there are some which appear to afford a glimpse of general structure.

Captain Flinders, in describing the position of the chains of islands on the north-west coast of Carpentaria, Wessell's, the English Company's, and Bromby's Islands, remarks, that he had "frequently observed a great similarity both in the ground plans, and the elevations of hills, and of islands, in the vicinity of each other, but did not recollect another instance of such a likeness in the arrangement of clusters of islands†." The appearances which called for this observation, from a voyager of so much sagacity and experience in physical geography, must probably have been very remarkable; and, combined with information derivable from the charts, and from the specimens for which we are indebted to Captain King and Mr. Brown,

* The following are the proportions assigned by Captain de Freycinet to the principal divisions of the globe.—*Voyage aux Terres Australes*, p. 107.

	French leagues square.	Proportions.
Asia	2,200,000	17
America	2,100,000	17
Africa	1,560,000	12
Europe	501,875	4
Australia	384,375	3

The most remote points from the coast of New South Wales, to which the late expeditions have penetrated, (and the interior has never yet been examined in any other quarter,) are not above 500 miles, in a direct line, from the sea; the average width of the island from east to west being more than 2000 miles, and from north to south more than 1000 miles.

† Flinders, v. ii. p. 246; and Charts, Plates 14 and 15.—King's Charts, Plate 4.

Fig 1

tinued) Fig. 2

Fig 3

Fig. 4.

Fig. 5

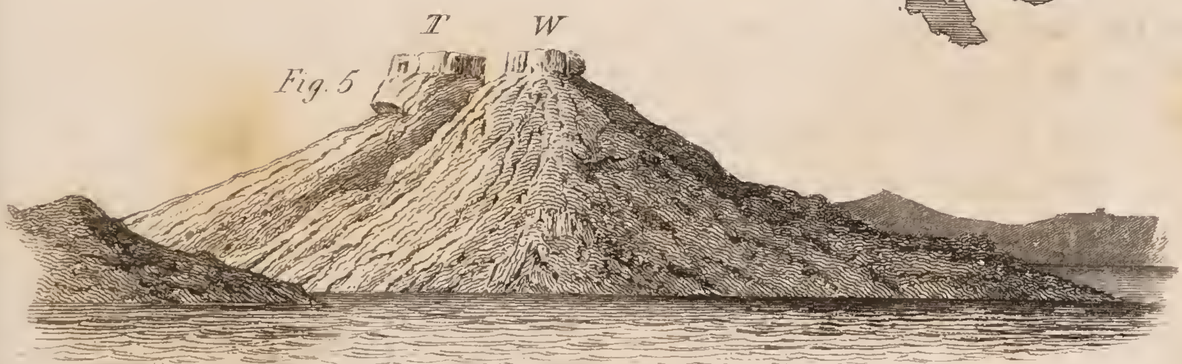


Fig 6

Brown, they would seem to point out the arrangement of the strata on the northern coasts of New Holland.

Of the three ranges which attracted Capt. Flinders's notice, (see the map, fig. 4, in Pl. I.*) the first on the south-east, (3,4,5, 6,7,) is that which includes the Red Cliffs, Mallison's Island, a part of the coast of Arnhem's Land, from Cape Newbold to Cape Wilberforce, and Bromby's Isles; and its length, from the main land (3) on the south-west of Mallison's Island, to Bromby's Isles (7) is more than fifty miles, in a direction nearly from south-west to north-east. The English Company's Islands, (2, 2, 2, 2,) at a distance of about four miles, are of equal extent; and the general trending of them all, Captain Flinders states (p. 233), is nearly N.E. by E., " parallel with the line of the main coast, and with Bromby's Islands."—Wessell's Islands, (1, 1, 1, 1,) the third or most northern chain, at fourteen miles from the second range, stretch out to more than eighty miles from the main land, likewise in the same direction.

It is also stated by Captain Flinders, that three of the English Company's Islands which were examined, slope down nearly to the water on their west sides; but on the east, and more especially the south-east, they present steep cliffs; and the same conformation, he adds, seemed to prevail in the other islands †. If this structure occurred only in one or two instances, it might be considered as accidental; but as it obtains in so many cases, and is in harmony with the direction of the ranges, it is not improbably of still more extensive occurrence, and would intimate a general elevation of the strata towards the south-east.

Now on examining the general map, it will be seen, that the lines of the coast on the main land, west of the Gulf of Carpentaria, between Limmen's Bight and Cape Arnhem,—from the bottom of Castlereagh Bay to Point Dale,—less distinctly from Point Pearce, lat. 14° 23′, long. 129° 18′, to the western extremity of Cobourg Peninsula,—and from Point Coulomb, lat. 17° 20′, long. 123° 11′, to Cape Londonderry, have nearly the same direction;—the first line being about one hundred and eighty geographical miles, the second more than

* The following is an explanation of this map:

A	Castlereagh Bay	1, 1 &c.—Wessell's Islands
B	Point Dale	2, 2 &c.—The English Company's Islands
C	Arnhem Bay	3 —— Red Cliffs
D	Melville Bay	4 —— Mallison's Island
E	Cape Arnhem	5 —— Cape Newbold
F	Caledon Bay	6 —— Cape Wilberforce
		7 —— Bromby's Islands.

† Flinders, vol. ii. p. 235.

three

three hundred, and the last more than four hundred miles, in length*. And these lines, though broken by numerous irregularities, especially on the north-west coast, are yet sufficiently distinct to indicate a probable connexion with the geological structure of the country; since the coincidence of similar ranges of coast with the direction of the strata, is a fact of very frequent occurrence in other parts of the globe†. And it is observable that considerable uniformity exists in the specimens, from the different places in this quarter of New Holland which have been hitherto examined; sandstone like that of the older formations of Europe occurring generally on the north and north-west coasts, and appearing to be extensively diffused on the north-west of the Gulf of Carpentaria, where it reposes upon primitive rocks‡.

The horn-like projection of the land, on the east of the Gulf of Carpentaria, is a very prominent feature in the general map of Australia, and may possibly have some connexion with the structure just pointed out. The western shore of this horn, from the bottom of the gulf to Endeavour Straits, being very low; while the land on the east coast rises in proceeding towards the south, and after passing Cape Weymouth, latitude

* It is deserving of notice, that the coast of Timor, the nearest land on the north-west, at the distance of about 300 miles, is also nearly straight, and parallel to the Coast of New Holland in this quarter: part of the mountainous range, of which that island consists, being probably more than 9000 feet high; and its length, from the north-eastern extremity to the S.W. of the adjoining island of Rottee, about 300 miles.--But, unfortunately for the hypothesis, a chain of islands immediately on the north of Timor, is continued nearly in a right line for more than 1200 miles, (from Sermatta Island to the south-eastern extremity of Java,) in a direction *from east to west.* This chain, however, contains several volcanoes, including those of Sumbawa, the eruption of which, in 1815, was of extraordinary violence. See R. Inst. Journal, vol. i. (1816), p. 248, &c.

At Lacrosse Island, in the mouth of Cambridge Gulf, on the north-west coast of New Holland, the beds rise to the N.W.: their direction consequently is from S.W. to N.E.; and the rise towards the high land of Timor. The intervening sea is very shallow.

† A remarkable case of this kind, which has not, I believe, been noticed, occurs in the Mediterranean; and is conspicuous in the new chart of that sea, by Captain W. H. Smyth. The eastern coast of Corsica and Sardinia, for a space of more than two hundred geographical miles being nearly rectilinear, in a direction from north to south; and, Captain Smyth has informed me, consisting almost entirely of granite, or, at least, of primitive rocks. The coast of Norway affords another instance of the same description; and the details of the ranges in the interior of England furnish several examples of the same kind, on a smaller scale.

‡ The coast lines nearly at right angles to those above mentioned—from the S.E. of the Gulf of Carpentaria to Limmen's Bight,—from Cape Arnhem to Cape Croker,—and from Cape Domett to Cape Londonderry,—have also a certain degree of linearity; but much less remarkable, than those which run from S.W. to N.E.

12° 30′,

12° 30′, is in general mountainous and abrupt; and Captain King's specimens from the north-east coast, show that granite is found in so many places along this line, as to make it probable that primitive rocks may form the general basis of the country in that quarter; since a lofty chain of mountains is continued on the south of Cape Tribulation, not far from the shore, throughout a space of more than five hundred miles. It would carry this hypothesis too far, to infer that these primitive ranges are connected with the mountains on the west of the English settlements near Port Jackson, &c., where Mr. Scott has described the coal-measures as occupying the coast from Port Stevens, about lat. 33° to Cape Howe, lat. 37°, and as succeeded, on the eastern ascent of the Blue Mountains, by sand-stone, and this again by primitive strata*:—But it may be noticed, that Wilson's Promontory, the most southern point of New South Wales, and the principal islands in Bass's Straits, contain granite; and that primitive rocks occur extensively in Van Diemen's Land.

The uniformity of the coast lines is remarkable also in some other quarters of Australia; and their direction, as well as that of the principal openings, has a general tendency to a course from the west of south to the east of north. This, for example, is the general range of the south-east coast, from Cape Howe, about lat. 37°, to Cape Byron, lat. 29°, or even to Sandy Cape, lat. 25°; and of the western coast, from the south of the islands which enclose Shark's Bay, lat. 26°, to North-west Cape, about lat. 22°.—From Cape Hamelin, lat. 34° 12′, to Cape Naturaliste, lat. 33° 26′, the coast runs nearly on the meridian. The two great fissures of the south coast, Spencer's, and St. Vincent's Gulfs, as well as the great northern chasm of the Gulf of Carpentaria, have a corresponding direction; and Captain Flinders (Chart 4.) represents a high ridge of rocky and barren mountains, on the east of Spencer's Gulf, as continued, nearly from north to south, through a space of more than one hundred geographical miles, between latitude 32° 7′ and 34°.—Mount Brown, one of the summits of this ridge, about latitude 32° 30′, being visible at the distance of twenty leagues.

The tendency of all this evidence is somewhat in favour of a general parallelism in the range of the strata,—and perhaps of the existence of primary ranges of mountains on the east of Australia in general, from the coast about Cape Weymouth† to

* Annals of Philosophy, June 1824.

† The possible correspondence of the great *Australian Bight*, the coast of which in general is of no great elevation, with the deeply-indented Gulf of Carpentaria,—tending, as it were, to a division of this great island into two,

to the shore between Spencer's Gulf and Cape Howe. But it must not be forgotten, that the distance between these shores is more than a thousand miles in a direct line;—about as far as from the west coast of Ireland to the Adriatic, or double the distance between the Baltic and the Mediterranean.—If, however, future researches should confirm the indications above mentioned, a new case will be supplied in support of the principle long since advanced by Mr. Michell*, which appears (whatever theory be formed to explain it,) to be established by geological observation in so many other parts of the world,—that the outcrop of the inclined beds, throughout the stratified portion of the globe, is every where parallel to the longer ridges of mountains,—towards which, also, the elevation of the strata is directed. But in the present state of our information respecting Australia, all such general views are so very little more than mere conjecture, that the desire to furnish ground for new inquiry, is, perhaps, the best excuse that can be offered for having proposed them.

Detailed List of Specimens.

The specimens mentioned in the following list have been compared with some of those of England and other countries, principally in the cabinets of the Geological Society, and of Mr. Greenough; and with a collection from part of the confines of the primitive tracts of England and North Wales, formed by Mr. Arthur Aikin, and now in his own possession. Captain King's collection has been presented to the Geological Society; and duplicates of Mr. Brown's specimens are deposited in the British Museum.

Rodd's Bay, on the East Coast, discovered by Capt. King, about sixty miles south of Cape Capricorn†.—*Reddish sandstone*, of moderately-fine grain, resembling that which in England occurs in the coal formation, and beneath it (mill-stone grit). A *sienitic compound*, consisting of a large proportion of

two, accords with this hypothesis of mountain ranges: but the distance between these recesses, over the land at the nearest points, is not less than a thousand English miles.—The granite, on the south coast, at Investigator's Islands,—and westward, at Middle Island, Cape Le Grand, King George's Sound, and Cape Naturaliste, is very wide of the line above mentioned, and nothing is yet known of its relations.

* On the Cause of Earthquakes.—Philosophical Transactions, 1760, vol. li. p. 566—585, 586.

† In Captain King's collection are also specimens found on the beach at *Port Macquarie*, and in the bed of the *Hastings River*, of common serpentine, and of botryoidal magnesite, from veins in serpentine. The magnesite agrees nearly with that of Baudissero, in Piedmont. (See Cleaveland's Mineralogy, 1st edition, p. 345.)

of reddish felspar, with specks of a green substance, probably mica;—resembling a rock from Shap in Cumberland.

Cape Clinton, between Rodd's Bay and the Percy Islands. —*Porphyritic conglomerate*, with a base of decomposed felspar, inclosing grains of quartz and common felspar, and some fragments of what appears to be *compact epidote;* very nearly resembling specimens from the trap rocks* of the Wrekin and Breeden Hills in Shropshire. Reddish and yellowish *sandy clay*, coloured by oxide of iron, and used as pigments by the natives.

Percy Islands, about one hundred and forty miles north of Cape Capricorn.—*Compact felspar* of a flesh-red hue, inclosing a few small crystals of reddish felspar and of quartz. This specimen is marked "general character of the rocks at Percy Island," and very much resembles the compact felspar of the Pentland Hills near Edinburgh, and of Saxony. Coarse *porphyritic conglomerate*, of a reddish hue. *Serpentine.* A trap-like compound, with somewhat the aspect of serpentine, but yielding with difficulty to the knife.—This specimen has, at first sight, the appearance of a conglomerate, made up of portions of different hues, purplish, brown, and green; but the coloured parts are not otherwise distinguishable in the fracture: —It very strongly resembles a rock which occurs in the trap-formation, near Lyd-Hole, at Pont-y-Pool, in Shropshire. *Slaty clay*, with particles of mica, like that which frequently occurs immediately beneath beds of coal.

Repulse Island, in Repulse Bay, about one hundred and twenty miles north-west of the Percy Islands.—Indistinct specimens, apparently consisting of decomposed *compact felspar*. A compound of quartz, mica, and felspar, having the appearance of re-composed granite.

Cape Cleveland, about one hundred and twenty miles north of Repulse Island.—Yellowish-gray *granite*, with brown mica; "from the summit of the hill." Reddish *granite*, of very fine grain; with the aspect of sand-stone. Dark gray

* By the terms *Trap*, and *Trap-formation*, which I am aware are extremely vague, I intend merely to signify a class of rocks, including several members, which differ from each other considerably in mineralogical character, but agree in some of their principal geological relations; and the origin of which very numerous phænomena concur in referring to some modification of volcanic agency. The term *Green-stone* also is of very loose application, and includes rocks that exhibit a wide range of characters;—the predominant colour being some shade of green, the structure more or less crystalline, and the chief ingredients supposed to be hornblende and felspar,—but the components, if they could be accurately determined, probably more numerous and varied, than systematic lists imply.

 porphyritic

porphyritic hornstone, approaching to compact felspar, with imbedded crystals of felspar.

Cape Grafton, about one hundred and eighty miles west of north from Cape Cleveland.—Close-grained gray and yellowish-gray *granite*, with brown mica. A reddish granitic stone, composed of quartz, felspar, and tourmaline.

Endeavour River, about one hundred miles west of north from Cape Grafton.—Gray *granite* of several varieties; from a peaked hill under Mount Cook and its vicinity. Granular *quartz-rock* of several varieties: and indistinct specimens of a rock approaching to *talc-slate*.

Lizard Island, about fifty miles east of north from Endeavour River.—Gray *granite*, consisting of brown and white mica, quartz, and a large proportion of felspar somewhat decomposed.

Clack Island, near Cape Flinders, on the north-west of Cape Melville, about ninety miles north-west of Lizard Island.—Smoke-gray micaceous *slaty-clay*, much like certain beds of the old red sand-stone, where it graduates into grey wacke. This specimen was taken from an horizontal bed about ten feet in thickness, reposing upon a mass of pudding-stone, which included large pebbles of quartz and jasper; and above it was a mass of sand-stone, more than sixty feet thick.—(Narrative, vol. ii. p. 26.)

Sunday Island, near Cape Grenville, about one hundred and seventy miles west of north from Cape Melville.—*Compact felspar*, of a flesh-red colour; very nearly resembling that of the Percy Islands, above mentioned.

Good's Island, one of the Prince of Wales's group, about latitude 10°, thirty-four miles north-west of Cape York.—The specimens, in Mr. Brown's collection from this place, consist of coarse-slaty *porphyritic conglomerate*, with a base of greenish-gray compact felspar, containing crystals of reddish felspar and quartz. This rock has some resemblance to that of Clack Island above mentioned.

Sweer's Island, south of Wellesley's group, at the bottom of the Gulf of Carpentaria.—A *stalactitic concretion* of quartzose sand, and fine gravel, cemented by reddish carbonate of lime; apparently of the same nature with the stem-like concretions of King George's Sound: (See p. 146). In this specimen the tubular cavity of the stalactite is still open.

The shore, in various parts of this island, was found to consist of red ferruginous matter, (*Bog-iron-ore ?*) sometimes unmixed, but not unfrequently mingled with a sandy calcareous stone; and in some places rounded portions of the ferruginous matter were enveloped in a calcareous cement.

Bentinck

Bentinck Island, near Sweer's Island.—A granular compound, like sand-stone recomposed from the debris of granite. *Brown hematite*, inclosing quartzose sand.

Pisonia Island, on the east of Mornington's Island, is composed of calcareous *breccia* and *pudding-stone*, which consist of a sandy calcareous cement, including water-worn portions of reddish ferruginous matter, with fragments of shells.

North Island, one of Sir Edward Pellew's group.—Coarse siliceous sand, concreted by ferruginous matter; which, in some places, is in the state of brown hematite. *Calcareous incrustations*, including fragments of madrepores, and of shells, cemented by splintery carbonate of lime.

Cape-Maria Island, in Limmen's Bight, was found by Mr. Brown to be composed principally of *sand-stone*. The specimens from this place, however, consist of gray *splintery hornstone*, with traces of a slaty structure; and of yellowish-gray *flint*, approaching to calcedony; with a coarse variety of cacholong, containing small nests of quartz crystals.

Groote Eylandt is composed of *sandstone*, of which two different varieties occur among the specimens. A quartzose *reddish sand-stone*, of moderately fine grain; and a coarse reddish compound, consisting almost exclusively of worn pebbles of quartz, some of which are more than half an inch in diameter, with a few rounded pebbles of calcedony. The latter rock is nearly identical with that of Simms's Island, near Goulburn's Island on the north coast.

Chasm Island, Winchelsea Island, and Burney's Island, are of the same materials as Groote Eylandt: and *sand-stone* was found also on the western shore of Blue-Mud Bay.

On the shore of the mainland, opposite to Groote Eylandt, a little north of latitude 14°, Mr. Brown observed the "common sandy calcareous stone, projecting here and there in ragged fragments."

Morgan's Island, in Blue-Mud Bay, north-west of Groote Eylandt, is composed principally of *clink-stone*, sometimes indistinctly columnar. But among the specimens are also a coarse *conglomerate* of a dull purplish colour,—including pebbles of granular quartz and a fragment of a slaty rock like potstone: the hue and aspect of the compound being precisely those of the oldest sand-stones. Reddish *quartzose sand-stone*, of uniform and fine grain. A concretion of rounded quartz pebbles, cemented by ferruginous matter, apparently of recent formation.

Round Hill, near Cape Grindall,—a prominence east of north from Blue-Mud Bay, was found by Captain Flinders to consist, at the upper part, of *sand-stone*. The specimens of

the rocks in its vicinity are, dark-gray *granite*, somewhat approaching to gneiss, with a few specks of garnet; and a calcareous, probably concretional stone, inclosing the remains of shells, with cavities lined with crystals of calcareous spar.

Mount Caledon, on the mainland, west of Caledon Bay, consists of gray *granite*, with dark brown mica in small quantity; and on the sides and top of the hill "large loose blocks of that rock were observed, resting upon other blocks."

A small island, near Cape Arnhem, is also composed of *granite*, in which the felspar has a blueish hue.

Smaller of the Melville Islands, north-east of Melville Bay*.—A botryoidal mass of ferruginous *oxide of manganese*, approaching to hematite; the fissures in some places occupied by carbonate of lime.

Melville Bay.—*Granite*, composed of gray and somewhat blueish felspar, dark brown mica, and a little quartz; containing minute disseminated specks of *molybdena*, and indistinct crystals of pale *red garnet*.

Red Cliffs, south-west of Arnhem Bay;—on the line of the first chain of islands mentioned by Captain Flinders. (See the Map, Pl. I., fig. 3.)—Friable *conglomerate*, of a full brick-red colour, consisting of minute grains of quartz, with a large proportion of ochreous matter.

Mallison's Island. (Map, fig. 4.)—The cliffs of this island are composed of a fissile primitive rock, on which sand-stone reposes in regular beds. The specimen of the former resembles *gneiss*, or *mica slate*, near the contact with granite: the *sand-stone* is thick-slaty, quartzose, of a reddish hue, with mica disseminated on the surfaces of the joints; and one face of the specimen is incrusted with quartz crystals, thinly coated with botryoidal hematite. Light gray *quartzose sand-stone* of a fine grain, with a thin coating of brown hematite, was also found in this island:—And a *breccia*, consisting of angular fragments of sandstone, cemented by thin, vein-like, coatings of dark brown hematite, was found there, in loose blocks at the bottom of perpendicular cliffs.—The specimen of this breccia is attached to a plate of granular quartz, and may possibly have been part of a vein.

The shore of Inglis's Island, the largest of the English Company's Range, (2. 2. 2. in the Map,) is formed of flat beds, of a slaty argillaceous rock, which breaks into rhomboidal fragments; but the specimen is indistinct. Ferruginous mases, probably consisting of *brown hematite*, come also from this island.

* The relative position of the islands and bays on this part of the coast is represented in the enlarged Map, Pl. I.

ASTELL'S ISLAND, north-east of Inglis's Isle. Very fine-grained grayish-white *quartzose sand-stone;*—identical with that of Mallison's Island, and very closely resembling some of the specimens from Prince Regent's and Hunter's Rivers.

Among the remaining islands of this range,—BOSANQUET'S, COTTON'S, and POBASSOO'S Isles, were found by Mr. Brown to consist, in a great measure, of *sand-stone*, of the same character with the specimens above-mentioned.

POBASSOO'S ISLAND, a small islet south-east of Astell's Isle. —Fine-grained, somewhat reddish *sand-stone*. Another specimen of sand-stone is friable, of a light flesh-red colour, and apparently composed of the debris of granite. A crystalline rock, consisting of greenish-gray hornblende, with a very small proportion of felspar (*Hornblende rock?*).—Fragment, apparently from a columnar mass, of a stone intermediate between clink-stone and compact felspar.

Such of the English Company's Islands as were examined by Captain Flinders, are stated by him to consist, in the upper part, of a grit, or *sand-stone*, of a close texture; the lower part being argillaceous, and stratified, and "separating into pieces of a reddish colour, resembling flat tiles." The strata dip to the west, at an angle of about 15°.

South-west bay of GOULBURN'S SOUTH ISLAND, two hundred and fifty miles west of the Gulf of Carpentaria, (Narrative, i. p. 64.)—Coarse-grained reddish quartzose *conglomerate* and *sand-stone;* resembling the older sand-stones of England and Wales, and especially the "mill-stone grit" beneath the coal formation. Fine grayish-white *pipe-clay;* of which about thirty feet in thickness were visible, apparently above the sand-stone last mentioned. Coarse-grained *ferruginous sand-stone*, containing fragments of quartz, from above the pipe-clay.—The appearance of the cliff from which these specimens were taken, is represented in the view of the bay on the south of Goulburn Island, (vol. i. p. 66); and a distant head in the view consists of the same materials.

SIMMS'S ISLAND, on the west of Goulburn's south Island, (Narrative, i. p. 70)—is composed of a reddish *conglomerate*, nearly identical with some of the specimens above mentioned.

The western side of LETHBRIDGE BAY, on the north of MELVILLE ISLAND, consists of a range of cliffs like those at Goulburn's Island; the upper part being red, the lower white and composed of *pipe-clay*. The western extremity of BATHURST ISLAND, between CAPE HELVETIUS and CAPE FOURCROY, is also formed of cliffs of a very dark red colour.

LACROSSE ISLAND, at the mouth of CAMBRIDGE GULF, about one hundred miles from Port Keats.—Reddish, very *quartzose* *sand-*

sand-stone; from a stratum which dips to the south-east, at an angle of about ten or fifteen degrees. Micaceous and *argillaceous* fissile *sand-stone*, of purplish and greenish hues, in patches, or occasionally intermixed;—precisely resembling the rock of Brecon, in South Wales, and, generally, the "old red sand-stone" of the vicinity of Bristol and the confines of England and Wales. Fine-grained thin-slaty *sand-stone*, resembling certain beds of the coal formation, or of the millstone grit, is found in large masses, under an "argillaceous cliff," on the north side of Lacrosse Island.

The specimens from the interior of Cambridge Gulf are from Adolphus Island, and consist of reddish and gray *sand-stone*, more or less decomposed.

Vansittart Bay, about one hundred and forty miles north-west of Cambridge Gulf.—Reddish quartzose *sand-stone*, or *quartz-rock*. Indistinct specimens of *green-stone*, with adhering quartz; apparently a primitive rock.

Port Warrender, at the bottom of Admiralty Gulf, about forty miles south-west of Vansittart Bay, (Narrative, vol. i. p. 322, 323.)—*Epidote* and *quartz*, in small crystals confusedly interlaced; apparently from veins, or nests, but unaccompanied by any portion of the adjacent rock.—The structure in one of these specimens approaches to the amygdaloidal. A compact greenish stone, with disseminated crystalline spots of epidote, and of quartz, and apparently consisting of an intimate mixture of those minerals, is also among the specimens from Port Warrender.

All these specimens are from detached water-worn masses at the foot of Crystal Head, on the south-west of the port. The summit of the head is flat and tabular, and the rocks in the vicinity are described by Captain King as consisting of siliceous sand-stone. *Calcedony*, apparently from amygdaloid of the trap formation, was also found at Port Warrender.

The epidote of this place is in general of a pale-greenish colour, but is mixed with, and sometimes appears to pass into, spots of a rich purplish-brown. The specimens resemble generally the epidote of Dauphiny and Siberia; but Mr. Levy, who has been so good as to examine them, informs me that the crystals exhibit some modifications not described either by Haüy, or by Mr. Haidinger in his paper on this mineral, and which are probably peculiar to this locality.

Water Island, on the west side of Cape Voltaire, at the south-west entrance of Port Warrender, is described (vol. i. p. 395) as consisting of quartzose *sand-stone;* as is also Kater Island, in Montagu Sound. And the same rock appears to occur

occur throughout the islands on this part of the coast.—(Narrative, i. p. 401.)

MONTAGU SOUND, about five-and-twenty miles south-west of ADMIRALTY GULF, (Narrative, i. p. 400.)—Grayish *granular quartz;* like that of the Lickey Hill, in Worcestershire. Fine-grained quartzose *sand-stone* of a purplish hue, resembling a rock on the banks of the Severn, near Bridgenorth. *Gray* and *reddish sand-stone;* apparently composed of the debris of granite, and very nearly resembling that of Simms's Island above mentioned.

HUNTER'S RIVER, falling into YORK SOUND, on the north-east side.—Somewhat coarse reddish-white *sandstone;* like that of the coal formation, and some varieties of millstone grit. Fine-grained, reddish-gray *quartzose sand-stone*, having the appearance of stratification, and resembling the rocks of Cambridge Gulf.

ROE'S RIVER, at the eastern termination of York Sound, (Narrative, i. p. 407, 408, 413,) runs between precipitous banks of *sand-stone*, in nearly horizontal strata, which rise to the height of three hundred feet.

CAREENING BAY, between York Sound and Prince Regent's River, (Narrative, vol. i. page 413; and vol. ii. page 43, &c.) —Crystalline *epidote*, and whitish quartz, apparently from a vein. Purplish-brown *epidote*, with small nests or concretions of green epidote and quartz; forming a sort of amygdaloid. *Conglomerate*, containing angular fragments of yellowish-gray quartz-rock, in a base of compact epidote. A nearly uniform greenish compound of *epidote* intimately mixed with *quartz*, also occurs at this place. Flat lamellar *calcedony*. Very fine-grained reddish-gray *quartzose sand-stone*, with traces of a slaty structure, resembling that of York Sound, and Cambridge Gulf, was found in the north-east end of this bay; and fine-grained *green-stone*, on the summit of the adjacent hills.

Several of these specimens are almost identical with those of Port Warrender; from which place Careening Bay is distant about sixty miles.

BAT ISLAND, (Narr. i. p. 432,) western entrance of Careening Bay.—*Quartz* from thin veins, with particles of an adhering rock, probably chlorite-slate. Quartz, containing disseminated *hematitic iron-ore* and *copper pyrites*. Quartz crystals, with calcedony, from nodules in *amygdaloid*. Quartz with *specular iron ore*. *Green-stone*, with calcedony and copper pyrites. A decomposed stone, probably consisting of *wacke*.—The specimens of trap-rocks from this place are from a cavern.

GREVILLE

Greville Island, near the entrance of Prince Regent's River.—Reddish, coarsely granular, *siliceous sand-stone;* in horizontal strata, intersected by veins of crystallized quartz*.

Half-Way Bay, within Prince Regent's River on the west of the entrance, near Greville Island.—*Hornblende rock?* nearly agreeing with that of Pobassoo's Island, on the north-west of the Gulf of Carpentaria, (See above, p. 141.) *Calcedony,* apparently from nodules in amygdaloid. *Greenish quartz,* approaching to heliotrope. Red somewhat slaty *jasper,* mixed with quartz and calcedony, and containing specular iron ore.

The specimens from this place much resemble some of those from Sotto i Sassi, in the Val di Fassa in the Tyrol, which I have seen in the collection of Mr. Herschel; and which consist of reddish jasper with calcedony, and a greenish flinty stone, like heliotrope,—the whole belonging to the trap-formation.

Point Cunningham, east of south from Cape Lévêque, and about one hundred and fifty miles south-west of Prince Regent's River.—Very compact and fine-grained reddish granular *quartz,* with a glistening lustre, and flat conchoidal fracture. This stone, though so compact in the recent fracture, has distinct traces of stratification on the decomposed surface, which is of a dull reddish hue. Bright red ferruginous *granular quartz,* (Eisen-kiesel?) with a glistening lustre, and a somewhat porous texture. A specimen of "the soil of the hills" at Cygnet Bay, consists of very fine reddish-yellow quartzose sand. A large rounded pebble, consisting of ferruginous *granular quartz,* of a dark purplish-brown colour, and considerable density, was found here; near a fireplace of the natives, by whom it is used for making their hatchets; with a fragment of a *calcareous incrustation,* like that of the west coast hereafter mentioned.

The next specimens in Captain King's collection,—a space of more than three hundred miles on this coast not having been examined by him,—are from Malus Island, in Dampier's Archipelago (See Narrative, vol. i. p. 56):—they consist of fine-grained *green-stone,* and what appears to be a basaltic rock, of amygdaloidal structure.

Dirk Hartog's Island, west of Shark's Bay.—A compound of rather fine-grained translucent quartzose *sand, cemented by carbonate of lime,* of various shades of reddish and yellowish gray. This stone has in some places the structure of a breccia; the angles of the imbedded fragments, which are from half an inch to two inches in diameter, being very distinct:—but in other parts, the fracture exhibits the appearance of

* Narrative, vol. ii. p. 53.

roundish

roundish nodules, composed of concentric shells,—or bags as it were, of calcareous matter, which vary in colour, and are filled with a mixture of the same substance and quartzose sand: and the spaces between these nodules are likewise occupied by a similar compound*.

The cementing lime-stone in the rock of this island, is very like some of the more compact portions of the stone of Guadaloupe, which contains the human skeletons, the hardness and fracture being nearly the same in both. The chief difference of these rocks seems to arise from the nature of the cemented substances; which, in the Gaudaloupe stone, being themselves calcareous, are incorporated, or melted as it were, into the cement, by insensible gradation†; while the quartzose sand, in that of Dirk Hartog's Island, is strongly contrasted with

* The following description given by the French naturalists of the rocks at Bernier's Islands, was probably taken from a large suite of specimens; and M. Péron states, (I. p. 204,) that it is strictly applicable to all the adjacent parts of the continent, and of the islands that were examined by the French voyagers:

"Le sable du rivage (de l'île Bernier) est quartzeux, mêlé d'une grande proportion de débris calcaires fortement atténués. La substance de l'île même se compose, dans ses couches inférieures, d'un grès calcaire coquillier, tantôt blanchâtre, tantôt rougeâtre, déposé par couches horizontales, dont l'epaisseur varie de 2 à 3 décimètres, (7 à 11 pouces,) et qui toutes étant très uniformes dans leur prolongement, pourroient offrir à la maçonnerie des pierres de construction naturellement taillées.

"Les coquilles incrustées dans ces massifs des roches sont presque toutes univalves; elles apartiennent plus particulièrement au genre Natice de M. de Lamarck, et ont les plus grands rapports avec l'espèce de Natice qui se trouve vivante au pied de ces rochers. Elles sont sans doute petrifiées depuis bien des siècles, car, outre qu'il est très difficile de les retirer intactes du milieu de ces grès, tant leur adhésion avec eux est intime, on les observe encore à plus de 50 mètres (150 pieds) au dessus du niveau actuel de la mer.

"Quelque régularité que ces bancs puissent affecter dans leur disposition générale, ils ne sont cependant pas tous homogènes dans leur substance; il est sur-tout une varieté de ces roches plus remarquable par sa structure. Ce sont des galets calcaires, agrégés dans une terre sablonneuse ocracée, qui leur est tellement adhérente, qu'on ne sauroit détruire cette espèce de gangue sans les briser eux mêmes. Tous ces galets affectent la forme globuleuse, et se composent d'un grand nombre de zones concentriques, qui se développent autour d'un noyau central d'un grès scintillant et brunâtre. Ces diverses couches ont à peine quelques millimetres d'épaisseur, et affectent des nuances agréables, qui varient depuis le rouge-foncé jusqu'au jaune-clair. La disposition générale de cette breche lui donne donc quelques rapports grossiers avec le granit globuleux de l'ile de Corse; et, par ses couches rubanées, concentriques, elle a quelque chose de l'aspect des Agathes-Onyx. ..... Les bancs de grès divers dont je viens de parler, constituent, à bien dire, la masse entière du pays qui nous occupe, &c."—Vol. i. p. 110. See also Freycinet, p. 187.

† See Mr. Kœnig's paper. Phil. Trans. vol. civ. (1814) p. 107, &c.

the calcareous matter that surrounds it*. But, wherever the imbedded fragments in the latter consist of lime-stone, their union with the cement is complete.

Rottnest Island, about four hundred and fifty miles south of Dirk Hartog's Island.—Indistinct specimens containing numerous fragments of shells, in a calcareous cement; the substance of these shells has at first sight the appearance of calcedony, and is harder than ordinary carbonate of lime.

The characters of the shells in Captain King's specimens from this place are indistinct; but the specimens at the Jardin du Roi, which, there is reason to suppose, have come from this part of the coast, contain shells of several species,—belonging among others to the genera, corbula, chama, cardium, porcellanea, turbo, cerithium. M. Prevost, to whom I am indebted for this account, observes, that notwithstanding the recent appearance of the shells, the beds which contain them are stated to occur at a considerable height above the sea: and he remarks that the aspect of the rock is very like that of the shelly deposite of St. Hospice, near Nice.

King George's Sound, on the south coast, east of south from Cape Leeuwin.—Beautifully white and fine *quartzose sand*, from the sea-beach. Yellowish gray *granite*, from Bald-head. Two varieties of a *calcareous rock*, of the same nature with that of Dirk Hartog's Island; consisting of particles of translucent quartzose sand, united by a cement of yellowish or cream-coloured carbonate of lime, which has a flat conchoidal and splintery fracture, and is so hard as to yield with difficulty to the knife. In this compound, there are not any distinct angular fragments as in the stone of Dirk Hartog's Island; but the calcareous matter is very unequally diffused.

A third form in which this recent calcareous matter appears, is that of irregular, somewhat tortuous, stem-like bodies, with a rugged sandy surface, and from half an inch to an inch in diameter; the cross fracture of which shows that they are composed of sand, cemented by carbonate of lime, either uniformly mixed throughout, or forming a crust around calcareous matter of a spongy texture; in which latter case they have some resemblance to the trunks or roots of trees.—A mass, which seems to have been of this description, is stated to have come from a height of about two hundred and fifty feet above the sea, at Bald-head, on the South Coast of Australia. These specimens, however, do not really exhibit any traces of organic structure; and so nearly resemble the irregular stalactitical concretions produced by the passage of calcareous or ferruginous

* Captain King informs me that the soundings in this part of the coast bring up a very fine quartzose-sand, like that cemented in the breccia.

solutions

solutions through sand*, that they are probably of the same origin; indeed the central cavity of the stalactite still remains open in some of the specimens of this kind from Sweer's Island in the Gulf of Carpentaria. The specimens from Madeira, presented to the Geological Society by Mr. Bowdich, and described in his notes on that island†, appear upon examination to be of the same character.—But there is no reason to suppose that the trunks of trees, as well as other foreign substances, may not be thus incrusted, since various foreign bodies, even of artificial production, have been so found. Professor Buckland has mentioned a specimen of concreted limestone from St. Helena, which contains the recent shell of a bird's egg‡; and M. Péron states that, in the concretional limestone rock of the South Coast of New Holland, the trunks of trees occur, with the vegetable structure so distinct as to leave no doubt as to their nature§.

XIX. *Notices respecting New Books.*

Preparing for Publication.

A TRANSLATION of the Baron Dupin's admirable Course of Mathematics applied to the Arts, and adapted to the State of the Arts in England, by Dr. Birkbeck, President of the London Mechanics' Institution, is in the press and will speedily be published in weekly numbers. Of all the writers of elementary books, the Baron Dupin seems the most fascinating. In the present work,—a transcript of his lectures,—he has succeeded in making the study of mathematics extremely amusing as well as instructive. Such a book has hitherto been a great desideratum in our language, and must be very acceptable to every student.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

G. B. Sowerby's Genera of Recent and Fossil Shells, No. XXIV.

This Number contains the following genera: *Oniscia*, *Pyramidella*, *Pyrula*, *Mactra*, *Lutraria*, *Tornatella*.

Oniscia is a new genus, distinguished from *Cassidaria* by its granulated inner lip, its very short, scarcely reflected canal, and its very singular general form. The species at present

* Tubular concretions of *ferruginous* matter, irregularly ramifying through sand, like the roots of trees, are described by Captain Lyon as occurring in Africa.—Lyon's Travels, Appendix, p. 65.

† Excursions in Madeira, 1825, p. 139, 140; and *Bull. des Sciences Naturelles*, vol. iv. p. 322.

‡ Geol. Trans. vol. v. p. 479.

§ Péron, ii. p. 75.

known are the *Strombus Oniscus* of Linnæans; a fossil species, *O. Cithara*; and two other recent ones, *cancellata* and *tuberculosa.*—The Lamarckian genus *Pyramidella* is modified so as to include all the shells which accord in the following characters: "Shell turrited, smooth, very rarely slightly ribbed, polished; volutions numerous, apex acute; aperture rather oblong acute, above, rather rounded at the base; outer lip slightly expanded, turned upwards at the base and united to the twisted columella; columella generally plaited."—The only shells admitted by Mr. Sowerby into the genus *Pyrula* of Lamarck, are such as are commonly called *Figs* in commerce, distinguished from all others by their thinness, and by the regularity of their form.—From the Linnæan *Mactræ* are separated the *Lutrariæ* and *Anatinæ*, the original generic appellation, however, being retained.

XX. *Proceedings of Learned Societies.*

ASTRONOMICAL SOCIETY OF LONDON.

June 9.—THE reading of the Rev. Fearon Fallows's paper, on the Small Transit Instrument, was concluded. Mr. Fallows's directions may be comprehended briefly in the following particulars: 1. Place the transit instrument as near the meridian as possible, and also substantial meridian-marks at a considerable distance both to the north and south. 2. The clock must be set forward to sidereal time, and its daily rate obtained. 3. Observations of pairs of high and low Greenwich stars must be made each evening *along* with others whose right ascensions are required. 4. The apparent right ascensions of the Greenwich stars must be computed up to the time of observation, or taken from the Nautical Almanac. 5. The azimuthal error must be found, if possible, by several of those pairs. Also, 6. The error of the clock at the transit of one of the Greenwich stars. 7. Reckon this error constant to every observation made during the same night. 8. The azimuthal error must be considered with a contrary algebraic sign for stars between the zenith (of the Cape) and the pole. 9. A proportional part of the daily rate must be applied to every observation from the first. 10. The error of each star from the true meridian must be computed from tables prepared for the purpose. 11. To the time of transit of each star, add the error of the clock (6), the proportional part of the daily rate (9), and the error from the meridian (10); the respective sums will give the true apparent right ascensions required. 12. Compute the

the sum of the corrections for precession, aberration, lunar and solar nutation, for every star at the time of observation, and apply each sum with a contrary algebraic sign to each true apparent right ascension; the result will give their mean right ascension for the beginning of the year. 13. Let a series of these for each star be registered, and the mean of each series (if the observations be good) may be expected to give the mean right ascensions at the beginning of the year, with considerable accuracy.—The author concludes with observing that frequent applications of the *level* to the axis of the instrument during a night's observations are indispensable.

The same evening there was read, "An Appendix to a former Paper on the Latitude of the Royal Observatory, by the Astronomer Royal." The author of this Appendix defines the latitude of a place to be the observed altitude of the centre of a small circle described by the pole-star, the state of the barometer and thermometer being given, *minus* the refraction due to that altitude. The last correction he regards as altogether arbitrary, and states that he employs Bradley's refractions. The observations of the last eighteen months at Greenwich, with the two circles, as described in a former paper, include 720 of the pole-star, from which the co-latitude deduced is 38° 31′ 21″·045.

There was next read, "A Summary of the Observations made for the Determination of the Latitude of the Observatory at Wilna, by M. Slawinski." The observations amount to 260, and were made in the months of October and November 1825. The author gives an account of his researches to determine the flexure in the repeating circle, and explains that his reductions are made both by means of the places of stars given in Bessel's Tables, and the positions announced in the Nautical Almanac for 1827. The latitude referred to the centre of the transit instrument is 54° 40′ 59″·09 deduced by comparison with Bessel, and 54° 41′ 0″·05 by comparison with the Naut. Alm. The greatest of these determinations is less by about 2″ than the latitude of the same observatory as given by M. Slawinski's predecessors *Poczobut* and *Sniadecki.*

The reading of M. Slawinski's paper was followed by that of one on "Micrometrical Observations of the Planet Saturn, made with Fraunhofer's large Refractor at Dorpat, by Professor Struve." These Observations were made with a refracting wire micrometer attached to Fraunhofer's large telescope now so well known: employing the power 540, Professor Struve describes both the instrument and the manner of observation; but it will be simply necessary here to record the

the results for the planet's mean distance, which are as below: viz.

1.	The external diameter of the external ring	=	40″·215
2.	internal do.		35 ·395
3.	external internal ring		34 ·579
4.	internal do.		26 ·748
5.	equatorial diameter of Saturn		18 ·045
6.	breadth of the external ring		2 ·410
7.	do. chasm between the rings		0 ·408
8.	do. internal ring .		3 ·915
9.	Distance of the ring from Saturn		4 ·352
10.	The equatorial radius of Saturn		9 ·022

The mean value of the inclination of the ring to the ecliptic is 28° 5′·9, with a probable error not exceeding 6′·9.

M. Struve has detected no trace of a division of the ring into many parts; but he observes that the outer ring is much less brilliant than the inner. The five longest-known satellites are readily distinguished, through Fraunhofer's telescope, even in the illuminated field. The 4th appears like a small disc, diameter 0″·75. M. Struve saw the 6th several times; but he has never seen the 7th; of whose existence indeed Schröeter entertains doubts.

The same paper also details the results of micrometrical measurements of Jupiter and its satellites, made with the same instruments and with the same power 540, or from thence to 600. The mean results at the mean distance of the planet from the earth, are,

1.	Jupiter's major axis		38″·442
2.	minor axis		35 ·645
3.	compression		0″·0728 or $\frac{1}{13{\cdot}71}$
4.	Mean diam. ♃'s 1st Sat.	.	1″·018
5.	2d	.	0 ·914
6.	3d	.	1 ·492
7.	4th	.	1 ·277

Schröeter and Harding have often imagined that they have detected a deviation of Jupiter from the elliptical form; and so thought Struve at first; but a closer examination enables him to explain the illusion. On March 7th this year, he thought the diameter which extended from 61°·4 lat. preceding S. to 61°·4 lat. following N., was obviously smaller than the ellipsis would allow. But the micrometric measurement proved that that was not the case. That evening the major axis, A, was 44″·75; the minor axis, B, was 41″·72; and the diameter in

in question taken with the same micrometer was $42''\cdot 34$. Calling this diameter x, and the latitude on the planet, l, we have $x = \frac{A \cdot B}{(A^2 \sin^2 l + B^2 \cos^2 l)^{\frac{1}{2}}}$, and the numerical result is $x = 42''\cdot 38$, differing only $0''\cdot 04$ from the measurement. Most probably it is the slanting position of the axes of the ellipse with regard to the vertical circle which causes this illusion.

Lastly, there was terminated on the same evening, an "Account of some Observations made with a twenty-feet Reflecting Telescope, by J. F. W. Herschel," Esq. Foreign Secretary of this Society. This valuable communication is divided into four sections. The first contains descriptions and approximate places of 300 new double and triple stars. The telescope with which the observations were made, is one of the "front view" construction; aperture 18 inches, focal length 20 feet. It was constructed in the year 1820, under the joint superintendence of Mr. Herschel and his venerable father. Its light with its full aperture enables it to reach the faintest nebulæ of the third class, while with an aperture of 10 or 12 inches it serves to define double stars of the first class of an average degree of closeness. Mr. Herschel briefly describes the method of differences employed in *sweeps* of the heavens, the modifications introduced into the process on account of Mr. Herschel's being deprived of the valuable assistance of his aunt, Miss Caroline Herschel, his classification and characteristics of the *magnitudes* of the stars from the 7th to the 20th inclusive, of which none of the three last can be seen with the least illumination, but comprehend the stars seen or suspected in resolvable nebulæ. Mr. H. then presents an example of the method in which the business of "a sweep" is conducted, and of the method of obtaining from it the approximate right ascensions and polar distances of the objects which it comprises; accompanied by several instructive remarks. The table exhibits, in eight columns, the approximate places of 321 new double and triple stars, for Jan. 1, 1825, with their estimated angles of position, distances, magnitudes, and other particulars. A great many of the double stars tabulated in this paper, exhibit the highly interesting and curious phænomenon of contrasted colours; in combinations of white and blue or purple, yellow, orange, or red, large stars, with blue or purple small ones: red and white combinations also sometimes occur, but with less frequency. In all these cases the excess of rays belonging to the less refrangible end of the spectrum falls to the share of the large star, and those of the more refrangible portion to the small. Another fact not less remarkable, and rendering highly probable some other relation than that of mere juxtaposition,

position, is, that though red single stars are common enough, no example of an insulated blue, green, or purple one has yet been produced.

The three remaining sections of this paper comprise observations of the second comet of 1825; an account of the actual state of the great nebula in Orion, compared with those of former astronomers; and observations of the nebula in the girdle of Andromeda. The account of the comet, and that of the great nebula in Orion, are accompanied with illustrative drawings, and the latter also with a kind of map representing the whole as a constellation, in which the parts are named agreeably to a rude resemblance which the whole nebula presents to the head, snout, and jaws of some monstrous animal. Aided by these drawings, the verbal account presents an instructively perspicuous description of the truly interesting phænomenon to which it relates.

XXI. *Intelligence and Miscellaneous Articles.*

ON CONTEMPORANEOUS METEOROLOGICAL OBSERVATIONS.

July 17, 1826. *Meteorological Observations made at Leighton-Buzzard. Latitude* 51° 54′ 56″. *Longitude* 2′ 39″ *west of Greenwich. Altitude* 306 *feet above the level of the sea.*

Hours.	Barometer. Height.	Barometer. Temp.	Thermom. In.	Thermom. Out.	Hygro. of Leslie.	Wind.	Observations.
A.M. 4	29·727	59	65	47	2	None.	No clouds.
5	·741	58	64	47	4	Do.	Do.
6	·750	58	64	50	10	Do.	Do.
7	.756	59	64	56	21	NW. small.	
8	·764	59·5	65	59	24	NW.	partially cl[y]
9	·768	61	66	63	35	NNW.	lt. br. few cl[s].
10	·770	61	66·5	65	33	NNW.	few sm. cl[s].
11	·775	61·5	66·5	65	31	NW.	Do.
12	·778	62	67·5	66	34	NW.	Do.
P.M. 1	·772	63	68	68	26	NNW.	Do.
2	·768	63·5	68·5	71	24	NNW.	Do.
3	·770	64	68·5	73	46	NW.	Do.
4	·770	64·5	69	73	41	NW.	Fine clear.
5	·768	65	70	74	51	NW.	Do.
6	·766	65	70·5	74	47	W.	Do.
7	·770	65	71	71	41	W.	Do.
8	·768	64·5	69	68	25	W.	Do.
9	·790	64	68	61	30	W.	Do.
10	·768	63	66	58	35		Do.

The Barometer used was made by Ramsden, with large cistern and adjusting float.

Sir,—In the Edinburgh Journal of Science, published in June, one of the articles of scientific intelligence is on Meteorology, originating with the Royal Society of Edinburgh; with a request that observations might be made in different parts of England, and on the Continent, on the barometer, thermometer, raingauge, &c. on the 17th of July at *every hour of the day.*

Being aware of the importance of such simultaneous observations for determining the relative altitude of the places of observation, as well as other points connected with the physical constitution of the atmosphere, and not seeing a notice of such an object in any other periodical work, or in the newspapers, I took the liberty to request the editor of the Courier to admit a notice to that effect in that paper, which was very liberally complied with.

In consequence of this notice I have received several communications from different parts of Great Britain; and although much praise is due to the gentlemen who took the trouble to register the hourly state of their instruments, it is to be regretted that many of them were only entered to two places of decimals instead of three, which in good barometers may be done; which, when connected with observations on the temperature of the air, and the degree of moisture by some instrument that will enable us to find the *dew point*, afford data for determining the relative heights of the stations with tolerable accuracy, and may serve, if occasionally repeated, to furnish a table of the real *altitude* of every place of consequence in Great Britain, and thereby supply the defect in all our geographical books; which, in defining the position of a place, at present only give us two out of three ordinates, viz. *latitude* and *longitude*.

And when it is known that actual sections of the surface of England have been obtained by levelling in almost all directions, I presume the labour in reducing the necessary information to one general list or table will not be great.

The task of observing *every hour* in the day appears to me more than necessary. The times of observation at the Royal Society's house in London are 9^h A.M. and 3^h P.M.; and as these are taken daily, opportunity is thus afforded to determine the heights of all places within a moderate distance from London.

I am, sir, yours truly,

B. Bevan.

P.S. Improved rules for calculating the height of places may be seen in Daniell's Meteorological Essays.

MAJOR LAING—AFRICA.

Dispatches have been received at the Colonial Office, dated 18th June, from Mr. Warrington, British Consul at Tripoli. These dispatches, we are delighted to state, announce the arrival of our intrepid countryman, Major Laing, at the great centre of African internal commerce, the long-sought city of Timbuctoo. The date of his arrival is not stated, but from the time he left Twat, it was probable that it took place about the beginning of February. The next caravan which arrives at Tripoli from Timbuctoo will bring us further accounts from our enterprising traveller regarding his future movements. If he proceeded down the river Niger as expeditiously as he could, we may soon expect to hear of his arrival in England. The reports of the dispersion of the caravan with which he was travelling after it had left Twat, and which had reached this country through a respectable channel, are thus, we rejoice to say, falsified. Inured to the African climate, and arriving at Timbuctoo early in the dry season, we consider every danger to Major Laing as over. The navigable current of the Niger will rapidly bear him, we think, to the Atlantic, through countries and powers deeply impressed with the majesty and fame of Great Britain. Two British travellers are at present in the heart of Northern Africa, to which they have advanced from opposite points. No later advices have been received from Clapperton than those which announced his arrival at Sackatoo, but by the arrival of the Dispatch man-of-war from the coast of Africa (the Bight of Benin) some previous dispatches from that traveller have been received, which are of considerable importance, as disclosing his route and progress to Sackatoo. On the 7th of March he was at Katangah, the capital of Yarba or Yarriba, a country bordering on Nyffe; from whence he was preparing to set out for Kiama, and from thence to Wauwa and Youri (distant four days journey from Wauwa); thus passing the places where our unfortunate countryman Park was lost. Katangah is stated to be 30 miles east of the Niger:—Important information he must, of course, have obtained there; but still more important information he of course obtained, and has no doubt generally transmitted to this country, in his advance to Katangah, and in his further advance northwards;—because in that route, and in the latter space, he must have crossed the Niger, and passed Nyffe, at that point where some will have it that the Niger turns east to the Nile of Egypt, and others that it empties itself into an inland lake.—There he must have received positive information whether the mighty Niger runs eastward, or continues its course, as we believe it does, southward,

ward, through that line of country yet unexplored, through which twenty mighty rivers, which enter the sea in the Delta of Benin, descend to the ocean. These points, we have no doubt, are in a great measure determined by the advices received from Clapperton; and probably the next *Quarterly Review* may favour us with a peep behind the curtain, which we wait with undiminished confidence to perceive drawn up.—*Glasgow Courier.*

CONFLAGRATIONS OF THE EARTH'S SURFACE.

——her wæs se dria sumor, and wilde fyr com on manega scyra.
Saxon Chronicle, A.D. 1077.

Among the natural phænomena of the present hot and dry summer, the numerous instances of the combustion of extensive tracts of bog and moor seem worthy of being investigated and recorded. We should have been glad to have been enabled to furnish our readers with authentic details; but shall at present only note the following, which have been mentioned in the public journals during June and July.

In Yorkshire the moors are stated to have been on fire in the neighbourhood of Sheffield and other places.

In Staffordshire, and in the Cambridgeshire Fens the ground is also said to be on fire.

In Scotland the moors have been extensively on fire, and some tracts of forest. The Dumfries Courier notices some disastrous conflagrations of this kind near Lochnagar.

In Holland the fens and moors have been burning to a considerable extent.

From Sweden there are accounts of extensive conflagrations of the moors and timber forests.

The *Etoile* under the head of "Petersburg, July 14," states that the marshes around that city are on fire: the ground being turf covered with furze.

We extract two accounts which appear to be the most descriptive and interesting.

"*Fires upon the Moors.*—The conflagrations upon the Moors still continue, and without abundant rain they are not likely to be speedily extinguished. Large tracts of sheep-walk have been entirely destroyed, and the hopes of those sportsmen who proposed to make the West-Riding Moors their field of fame are annihilated. The fires have not only spread over a wide extent of surface, but burn to a great depth, consuming not only the moss but the peat underneath. In some places, where they reach a soft substratum, they run to a great length under ground, and break out at the surface at other spots. The flames and smoke together, present a most formidable

 appearance

appearance from any of the high grounds from which a view of them can be obtained.—As some particular account of the extent of the devastation must be interesting to our readers, we have taken pains to ascertain the real state of the conflagrations; and the following is the result: Hawkesworth Moor is entirely destroyed. On Ilkley Moor 500 acres are burnt. There is no hope of any part of Bingley Moor being saved. Burley Moor is on fire, and is partly consumed. Thornton Moor is entirely destroyed and with it all the young plantations, which cost upwards of 2000*l.* in planting. Oaksworth Moor is entirely burnt. Ovenden Moor, Holme Moss, Burnsall Fell, Hebden and Grassington Moors, are on fire; and unless, as we have already said, incessant and heavy rain should speedily fall, every one of these must be entirely consumed. We have no doubt, that lightning is the cause of the conflagrations."—*Bradford Courier.*

" *The Moors.*—Notwithstanding the tract of high land which the sportsmen of Sheffield denominate the Moors lies within ten miles north-west of Sheffield, we believe little real information has been given to the public, respecting the work of devastation now going on. On the evening of Monday week last, it will be remembered that the atmosphere presented a most extraordinary appearance. As the sun was setting in the west, immense clouds burdened with electric fluid, arose in the east, and supernatural darkness appeared to anticipate the declining day. The peat and vegetable soil of which the Moors are composed having been prepared for ignition during the dry weather, by the most inconsiderable natural or artificial cause took fire, from the lightning at this time. The heath on the west side of the plantations belonging to James Rimington, Esq. first suffered from the destructive element; and on the following day it was discovered that the part of the Moor called Hobson Moss, had taken fire in different places, and was burning to the extreme depth of the soil. During the whole of the last fortnight the course of destruction has been unchecked; and the surface of ground now burning cannot be estimated at much less than 8,000 acres. The appearance of the land is highly interesting. The smoke is discovered before entering Bradfield; and passing that village, two miles towards Broomhead Hall, the west side of the Moors sends forth immense clouds of vapour, through which the flames occasionally arise. The birds, which appear to be tolerably plentiful, and strong on the wing, may be observed rising from the burning ground, and seeking shelter in more distant parts. We have heard many opinions ascribing the calamity to incendiaries; but we think, from the great distance which exists

between

between the parts of the Moors which are now burning, that the causes of the fire may be discovered in the extraordinary dryness of the season, and in the lightning which we have before alluded to."—*Sheffield Independent.*

The occurrence of so many cases of combustion of the earth's surface at so many points distant from each other, during the same season, will probably lead to some general inquiry into the subject. With this view we have thus briefly called the attention of our readers to it; and shall further refer them to some instances probably of similar phænomena in former times, recorded in the Saxon Chronicle, to which our attention has been directed by a friend.

"A.D. 1032. This year appeared that wild fire, such as no man ever remembered before, which did great damage in many places."

"A.D. 1048. The wild fire in Derbyshire and elsewhere did much harm."

"A.D. 1077. This year also was the *dry summer: and wild fire came upon many shires*, and burned many towns; and also many cities were ruined thereby."

Results of a Meteorological Journal for July 1826, kept at the Observatory of the Royal Academy, Gosport, Hants.

General Observations.

The first part of this month was dry, but during the latter part we had frequent showers of rain, which were very refreshing and beneficial to the fruits and vegetation after so long a drought. Dry and hot weather has not only been experienced in this country for the last two months, but in several more northerly parts of Europe, as Denmark, Sweden, &c., also in America. Although we have had several hot sultry days and nights this month, yet the heat in the sun's rays has not been so great as at the close of last month: but from the dryness of the spring and summer so far, we do not recollect having seen the roads more pulverized than they are at present. The 8th was the warmest day, when the *maximum* temperature in the shade, with light airs from the South, was 81 degrees. On the 23rd the *maximum* temperature in the shade was only 60 degrees. This difference of 21 degrees in the temperature of these two days, was occasioned by the falling of three quarters of an inch of rain in the night of the 22nd, which had cooled the surface of the earth; also by the then condensing state of the atmosphere, and the setting in of a strong gale from due North.

The mean temperature of the external air this month, is $2\frac{1}{3}$ degrees

degrees higher than the mean of July for the last ten years. The mean temperature of spring water has increased nearly two degrees this month.

In this neighbourhood the wheat harvest was commenced generally by the 24th, and is now (August 2nd) nearly all got in. From almost all parts of the country there are satisfactory accounts of the good quality and abundant crops of wheat. The barleys and oats on high lands and light soils are said to be stunted, and generally deficient, in consequence of the long drought.

During the whole month insects of the genus *Coccinella*, or Lady-bird, were very numerous here, and in the neighbouring towns: they increased in number till the 20th, when they nearly covered the railings around the fortifications; they were also on the ground and in the fields and gardens in great numbers. They were variously spotted, some had only two spots, some seven, and others eleven; and they were differently coloured*. In the hottest sunny days they were much on the wing, and were more numerous than the common flies. They are not injurious to vegetation, as they destroy the *animalculæ* thereon, and generally feed on grass and on the blight upon fruit-trees. They prefer being in the sun's rays to shady places; but they can endure the most rigorous weather, perhaps from their coleopterous state, several of the seven-spotted ones having survived the inclemency of the last winter in an open garden. The *larvæ* of Lady-birds do not thrive, except in a long continuance of dry and warm summer weather. It is now about eight years since they last swarmed here and throughout this county, the summer of 1818 having been remarkably hot and dry.

The atmospheric and meteoric *phænomena* that have come within our observation this month, are one parhelion, one lunar and two solar halos, eleven meteors, vivid lightning throughout the nights of the 4th and 30th, which overspread the whole hemisphere, and enlightened the attenuated parts of the passing clouds; thunder on the 14th and 30th; and three gales of wind; two from S.W., and one from the North.

* *C. 2-punctata, 7-punctata, 11-punctata,* and some species of the section with a black ground, or of that with white spots on a red ground, are doubtless intended by our respected correspondent. We are informed by Mr. Haworth that the 7-*punctatæ* have prevailed in a similar manner in the corn-fields in Norfolk, where their larvæ were so abundant in July, as to be trodden under foot at every step: whereas in a former year mentioned in Mr. Haworth's paper on *Coccinella* in the Transactions of the Entomological Society, it was the species 2-*punctata* that so remarkably abounded.

Their eating grass seems questionable: See the abovementioned paper by Mr. Haworth.—Edit.

Numerical

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·37,	July 26th—Wind N.E.
	Minimum	29·60,	Ditto 21st—Wind N.

Range of the mercury . . 0·77.

	Inches.
Mean barometrical pressure for the month	29·982
——— for the lunar period ending the 15th inst. .	30·230
——— for 16 days, with the Moon in North declin.	30·125
——— for 13 days, with the Moon in South declin.	30·335
Spaces described by the rising and falling of the mercury	3·670
Greatest variation in 24 hours	0·400
Number of changes	23·

Thermometer	Maximum	81°,	July 8th—Wind S.
	Minimum	51	Do. 16th—Wind NW.

Range	30
Mean temp. of the external air	66·84
——— for 31 days with the Sun in Cancer	67·94
Greatest variation in 24 hours	27·00
Mean temp. of spring water at 8 o'clock A.M. . . .	52·47

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air .	61 in the evening of the 23d.
Greatest dryness of ditto . . .	46 in the aftern. of the 19th [& 25th.
Range of the index	15
Mean at 2 o'clock P.M. . . .	52·5
—— at 8 o'clock A.M. . . .	55·3
—— at 8 o'clock P.M. . . .	56·4
—— of three observations each day at 8, 2, and 8 o'clock	54·7

Evaporation for the month	4·35 inch.
Rain in the pluviameter near the ground .	1·605
Rain in ditto 23 feet high	1·495

Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 7; fine, with various modifications of clouds, 16; an overcast sky without rain, $5\frac{1}{2}$; rain, $2\frac{1}{2}$.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
20	15	28	0	24	20	14

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
4	$2\frac{1}{2}$	$\frac{1}{2}$	6	2	11	3	2	31

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of* Dr. BURNEY *at* Gosport, Mr. J. CARY *in* London, *and* Mr. VEALL *at* Boston.

Gosport, at half-past Eight o'Clock, A.M.						Clouds.									Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Evaporation.	Rain near the ground.	Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
July 1	30·16	66	51·60	56	SE.	1	1	1	...	1	...	1	...	0·350	30·18	29·56	70	73	62	68·5	...	...	Cloudy	Cloud. rain, thr. & lg.	calm
2	30·26	68	......	54	N.	1	...	1	...	1	...	...	...	......	30·29	29·65	70	79	67	70	...	1·44	Fine	Fine [p.m.	calm
3	30·25	69	......	53	SE.	1	...	1	...	...	...	...	0·45	......	30·27	29·70	69	78	70	72	...	...	Cloudy	Fine	SW.
4	30·10	74	......	53	SE.	1	...	1	...	1	1	...	...	......	30·10	29·55	71	79	69	72·5	...	...	Fine	Fine Ther.	E.
● 5	29·90	73	......	55	SW.	1	...	1	...	1	...	...	...	......	29·94	29·20	75	79	71	75	...	...	Fine	Fine, 5 p.m. 81	W.
6	29·96	73	52·00	53	SE.	1	1	1	...	1	1	...	·50	......	29·99	29·30	69	76	72	73·5	...	...	Fine	Fine, 4 p.m. 83	W.
7	29·87	74	......	54	SW.	1	1	1	...	1	1	1	...	·010	29·88	29·20	73	78	70	73	...	...	Fair	Fine, rain a.m. and	S.
8	29·71	74	......	54	S.	1	1	1	...	...	1	1	...	......	29·78	29·11	72	75	68	71	...	·20	Cloudy	Fine [p.m.	SW.
9	29·71	73	......	53	SE.	1	1	1	...	1	1	...	·40	......	29·78	29·14	72	78	67	70	...	...	Cloudy	Cloudy, rain p.m.	calm
10	29·82	72	......	52	SW.	1	1	1	...	1	1	...	...	......	29·89	29·22	68	72	68	68	...	·06	Cloudy	Fine	W.
11	29·92	72	......	51	W.	1	1	1	...	1	1	1	...	......	29·97	29·30	69	71	67	68	...	...	Cloudy	Fine	W.
☽ 12	29·94	69	......	57	SW.	...	...	1	...	...	1	1	·30	......	29·86	29·35	66	68	68	63·5	...	...	Cloudy	Rain	W.
13	29·74	67	......	59	SW.	1	...	1	...	1	...	1	...	·050	29·77	29·10	68	70	64	70	...	·10	Showers	Cloudy, rain p.m.	W.
14	29·80	68	52·20	56	W.	...	1	1	...	1	1	...	...	......	29·85	29·25	65	71	65	64	...	·25	Fair	Cloudy, do.	SW.
15	29·90	69	......	56	W.	1	1	1	...	1	1	1	·30	·020	29·94	29·38	65	71	61	64	...	·04	Fair	Cloudy	W.
16	29·86	64	......	59	SW.	...	1	1	...	1	1	1	...	·065	29·85	29·27	61	64	59	60·5	...	...	Rain	Cloudy	W.
17	30·02	60	......	55	NW.	1	...	1	...	1	...	...	...	......	30·05	29·52	60	69	61	60·5	...	...	Fair	Fine	W.
18	30·03	69	......	53	NW.	1	1	1	...	1	1	...	·40	......	30·05	29·50	62	70	63	62	...	...	Cloudy	Cloudy	W.
○ 19	30·00	66	......	52	NW.	1	...	1	...	1	1	...	...	......	30·05	29·45	64	70	60	63	...	...	Fair	Cloudy	W.
20	30·08	67	52·75	53	W.	1	...	1	...	1	1	1	...	·210	30·03	29·53	61	67	61	62·5	...	...	Cloudy	Cloudy	W.
21	29·68	65	......	55	W.	...	...	...	...	1	1	1	·40	·055	29·70	29·10	62	65	59	63	...	·11	Showers	Fine, rain a.m.	NW.
22	29·77	60	......	55	NE.	1	1	1	...	1	1	1	...	·740	29·91	29·40	61	65	57	63·5	...	·03	Fair	Cloudy, do. p.m.	E.
23	29·94	56	......	60	N.	...	1	1	...	...	1	1	...	·100	30·07	29·63	56	58	52	61	...	·11	Rain	Stormy	E.
24	30·03	59	......	60	N.	...	1	1	...	...	1	1	·30	·005	30·11	29·72	54	68	64	63·5	...	...	Fair	Cloudy	E.
25	30·14	63	......	53	N.	1	...	1	...	1	...	...	...	......	30·20	29·85	67	70	62	62	...	...	Fair	Cloudy	SE.
26	30·25	65	53·00	57	NE.	...	...	...	...	1	1	...	...	......	30·32	29·90	63	68	56	61	...	...	Fair	Fine	calm
☾ 27	30·33	62	......	58	NE.	...	...	1	...	1	...	...	·50	......	30·33	29·80	60	67	60	62·5	...	...	Fair	Cloudy	calm
28	30·23	67	......	58	NE.	...	...	1	...	1	...	...	...	......	30·27	29·88	63	69	61	62	...	...	Fair	Fine Ther.	calm
29	30·15	66	......	56	NE.	1	...	1	...	...	...	...	...	......	30·15	29·58	65	71	61	69	...	...	Fair	Fine, 3½ p.m. 80	calm
30	30·09	67	......	57	SE.	...	...	...	...	...	...	...	...	......	30·10	29·48	70	78	74	72	...	...	Fine	Fine, extre. heat 87	W.
31	29·98	73	53·25	58	SE.	...	1	1	...	1	1	1	·80	......	30·05	29·37	75	81	69	74·5	...	...	Fine	Fine, do. 90	S.
Aver. :	29·988	67·42	52·47	55·3		20	15	28		24	20	14	4·35	1·605	30·02	29·45	66	71	64	66·6	2·359	2·34		[thr. & lg. at nt.	

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

30th SEPTEMBER 1826.

XXII. *On the Method of the Least Squares.* *By* J. Ivory, *Esq. M.A. F.R.S.**

I HAVE already treated of the method of the least squares in this Journal for January 1825, and the two following months. By altering a little an idea first suggested by Cotes, we may represent the errors of observation by the parts of a rigid line, the positive errors being on one side, and the negative errors on the other side, of a common fulcrum; and if we append to the extremities of the errors, weights proportional to the coefficients of the correction in the equations of condition, the equilibrium of the weights is the rule of the least squares, and determines the most advantageous value of the correction sought. When there are several elements to be corrected simultaneously, the coefficients of the different corrections in the equations of condition will form so many separate sets of weights, and the levers must be *in equilibrio* whichever set is appended; by which means we obtain as many equations as there are unknown quantities to be found. This method is at least precise and free from every thing conjectural or tentative. It is likewise founded on just principles; for every coefficient in the equations of condition has its due influence in the quantity of the result. But it must be allowed that the introducing of the properties of the lever and of equilibrium in a demonstration of this kind is not altogether unexceptionable. Such considerations would not be necessary if the truth to be proved were entirely disengaged from what is foreign to it. It is however extremely desirable that a method, which is of great practical utility, should be clearly and simply deduced from the real principles alone concerned; because it is only when this is done that the method can be fully understood, and that we can apply it with confidence and without danger of mistake. My intention in returning to this subject is to attempt an explanation of the ground of the method of the least squares that may in some degree answer the description here given.

* Communicated by the Author.

In the most simple case of only one element the equations of condition are as follows; viz.

$$e = a x - m$$
$$e' = a' x - m'$$
$$e'' = a'' x - m''$$
&c.

in which the coefficients of the correction x, viz. a, a', a'', &c. are all positive. Now it is manifest that the errors e, e', e'', &c. depend upon the correction x; in so much that when any particular value is assigned to x, all the errors are immediately determined. It appears therefore that there can exist no reason for preferring one value of x to another, except the nature of the errors, or the general character impressed upon them on the supposition that the experiments are skilfully executed. When the law of the errors is fulfilled, and when, besides their quantity is confined within the least possible limits, the problem is solved, and we have found that value of the correction which must be preferred to every other. What then is the general character of the errors of a set of experiments made for the purpose of ascertaining the quantity of some physical magnitude, or of approximating to it more nearly than had been done before? We may suppose at least, that the experiments are liable only to irregular and fortuitous errors; that every cause tending to make the results of observation incline more to one side than to another, has been carefully investigated and removed; and in short, that the errors contain no constant part common to them all. What is here said must not be understood exclusively of the case when every error contains a part of the same magnitude affected with the same sign; the principle evidently extends to all cases when the errors contain parts affected with the same sign although unequal in magnitude, provided these parts are necessarily connected with one another so that they must exist simultaneously. The point does not turn upon the equality or inequality of the parts, but upon this, That the errors of separate experiments must be independent on another and subject to no determinate law. In a repetition of the same experiment it is universally the practice to take the arithmetical mean of all the observed quantities, as much more exact than any particular result. Now this rule is founded on the independence of the experiments and of the errors to which they are liable; whence it follows that the errors in excess may be expected to balance those in defect; since no reason can be assigned why the amount of one should be different from the amount of the other. When the quantity to be observed varies from one experiment to another, always containing, however, the

the same elementary quantities sought, the principle of the independence of the experiments will still hold good; we must still suppose that the error committed in one case has no influence whatever on the error committed in any other case; and I shall prove that this principle, combined with the equations of condition, leads necessarily to the method of the least squares, of which the rule for the arithmetical mean is only a particular case.

Multiply all the terms of every equation of condition by the coefficient of x; then, having added all the results, we shall get

$$S(ae) = x \times S(a^2) - S(am),$$

the symbols used for the sake of abridging being thus explained, viz.

$$S(ae) = ae + a'e' + a''e'' + \&c.$$
$$S(a^2) = a^2 + a'^2 + a''^2 + \&c.$$
$$S(am) = am + a'm' + a''m'' + \&c.$$

Find x from the equation just obtained, then

$$x = \frac{S(am)}{S(a^2)} + \frac{S(ae)}{S(a^2)},$$

and by substituting this value in the expressions of the errors, we further obtain,

$$e = -m + a\,.\,\frac{S(am)}{S(a^2)} + a\,.\,\frac{S(ae)}{S(a^2)},$$
$$e' = -m' + a'\,.\,\frac{S(am)}{S(a^2)} + a'\,.\,\frac{S(ae)}{S(a^2)},$$
&c.

All the errors as well as x now depend upon one and the same quantity, namely, $S(ae)$. If we make this arbitrary quantity go through every gradation of magnitude, we shall obtain all the possible systems of the errors and every possible value of x. Let ε, ε', ε'' &c. denote the particular set of errors found by the condition, $S(a\varepsilon) = 0$;
which equation may be otherwise written thus,

$$\varepsilon \frac{d\varepsilon}{dx} + \varepsilon' \frac{d\varepsilon'}{dx} + \varepsilon'' \frac{d\varepsilon''}{dx} + \&c. = 0,$$

and it determines the minimum of the expression $\varepsilon^2 + \varepsilon'^2 + \varepsilon''^2 + \&c.$: it also fulfils the equilibrium of the weights a, a', a'' &c. as noticed in the beginning of this paper. Now put ξ for the particular value of x when the errors are ε, ε', ε'' &c.; then, by making $S(ae) = 0$ in the foregoing formulæ, we get

$$\xi = \frac{S(am)}{S(a^2)},$$
$$\varepsilon = -m + a\,.\,\frac{S(am)}{S(a^2)},$$
$$\varepsilon' = -m' + a'\,.\,\frac{S(am)}{S(a^2)},$$
$$\varepsilon'' = -m'' + a''\,.\,\frac{S(am)}{S(a^2)},$$
&c.

These

These expressions show that ε, ε', ε'', &c. are independent of one another. Every one is singly derived from the quantities of its proper experiment, without being influenced by the magnitudes of the rest. Let us now suppose that $S(a\,e)$ has any arbitrary value; then, according to the foregoing formulæ, we shall have,

$$\begin{aligned} x &= \xi + \frac{S(a\,e)}{S(a^2)}, \\ e &= \varepsilon + a\,.\,\frac{S(a\,e)}{S(a^2)}, \\ e' &= \varepsilon' + a'.\,\frac{S(a\,e)}{S(a^2)}, \\ e'' &= \varepsilon'' + a''.\,\frac{S(a\,e)}{S(a^2)}, \\ &\text{\&c.} \end{aligned}$$

It is evident, from these expressions that x and all the errors e, e', e'' &c. depend upon one and the same arbitrary quantity $S(a\,e)$. If we take the value of $S(a\,e)$ in the formula for e, and substitute it in the expressions of e', e'' &c. we shall get,

$$\begin{aligned} e &= \varepsilon + \frac{a}{a}(e - \varepsilon) \\ e' &= \varepsilon' + \frac{a'}{a}(e - \varepsilon) \\ e'' &= \varepsilon'' + \frac{a''}{a}(e - \varepsilon) \\ &\text{\&c.} \end{aligned}$$

Which proves that all the errors are determined when one only is known. The errors therefore are not independent on one another; and this is true of every possible system, excepting only the system ε, ε', ε'' &c. which is deduced from the rule of the least squares. It is therefore this last system of errors alone that can occur in a set of experiments or observations in which there exists no bias tending regularly one way, and where the error in one case is supposed to have no influence whatever on the error in any other case.

If we make the coefficients a, a', a'', &c. all equal to one another, or rather all equal to unit, we shall have the case of a repetition of the same experiment or observation, viz.

$$\begin{aligned} e &= x - m \\ e' &= x - m' \\ e'' &= x - m'' \\ &\text{\&c.} \end{aligned}$$

where x represents the unknown magnitude to be found, and m, m', m'', &c. the experimental values of it. Here the only system of independent errors is determined by the condition $S(\varepsilon) = 0$, or,

$$\varepsilon + \varepsilon' + \varepsilon'' + \&c. = 0,$$

which is equivalent to the rule of the arithmetical mean. In every other system the errors have a constant part common to them all, as is evident from what the general expressions become in the particular case under consideration, viz.

$$\begin{aligned} e &= \varepsilon + (e - \varepsilon) \\ e' &= \varepsilon' + (e - \varepsilon) \\ e'' &= \varepsilon'' + (e - \varepsilon) \\ &\&c. \end{aligned}$$

Such a set of errors cannot occur unless by the operation of some preponderating cause tending to augment or diminish every error in an equal degree. If there be no bias in the experiments, we must necessarily adopt the only system in which every error is singly determined by its proper quantities independently of all the other errors. The rule of the arithmetical mean is therefore only a particular inference from the general reasoning we have been explaining.

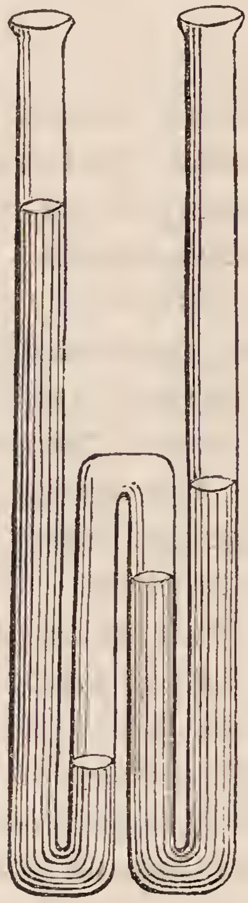
On account of its great practical utility, the method of the least squares has been the subject of much discussion on the continent. In order to prove it recourse has been had to the doctrine of probabilities and the most abstruse researches of the algebraic calculus. But the real grounds of the method are undoubtedly, the general character of the errors, and the properties of the equations of condition. From this last consideration we readily deduce that there is only one system of errors, or perhaps in particular circumstances, only a certain number of systems, that can possibly be consistent with the nature of the experiments. Viewed in this light, the method of the least squares is separated from the laws of chance, and is made to depend on very simple and elementary principles. The application of probability to physical researches rests on other grounds; namely, the general expression of the chance of an error so modified as to lead to rules sufficiently simple for application in practice. Although I have here confined myself to the most simple case of only one correction, yet the principles laid down are general, and are readily applied to several independent corrections by a repetition of similar reasoning.

Sept. 4, 1826. J. Ivory.

XXIII. *On*

XXIII. *On a Syphon Hydrometer, and its Use in finding the Temperature of Water at the greatest Density.* By Mr. Henry Meikle*.

THIS hydrometer consists of a glass tube, open at both ends, and bent into a sort of double syphon, having four parallel legs; so that the open ends are pointed in the same direction, or upwards, as in the annexed figure.



The manner of using it is very simple: Let one of the ends be stopped with the finger or with a cork, and water be poured into the other. This fluid will only rise a small way into the second leg, because of the included air. Next stop the other orifice, and open the one first closed; and having poured into the latter the liquid whose specific gravity is to be tried, open the top of the water-tube; then the instrument being held upright, the two liquids will arrange themselves so as to press equally on the included air. Now this pressure will be measured by the difference in the heights of the two columns of either liquid multiplied by its specific gravity. So that by dividing the difference of the two columns of water by the difference of those of the other liquid, we obtain the specific gravity of the latter; that of water being unity†.

The difference between the columns, which is the effective column, may be measured by applying any scale of small equal parts; or the glass might for greater safety be attached to a graduated board or plate, and this furnished with verniers, &c. Some little attention must be paid to the quantities of the liquids employed, for the longer the columns are the result will be the more certain; but no great nicety is required as to the precise quantity of either liquid. It is plain that the expansion of the glass or its capillary action have nothing to do with the result. Nor can the expansion of the scale have any influence; because the ratio of the columns is not altered thereby. Only if the temperature differ from the standard, as from 40° Fahr.

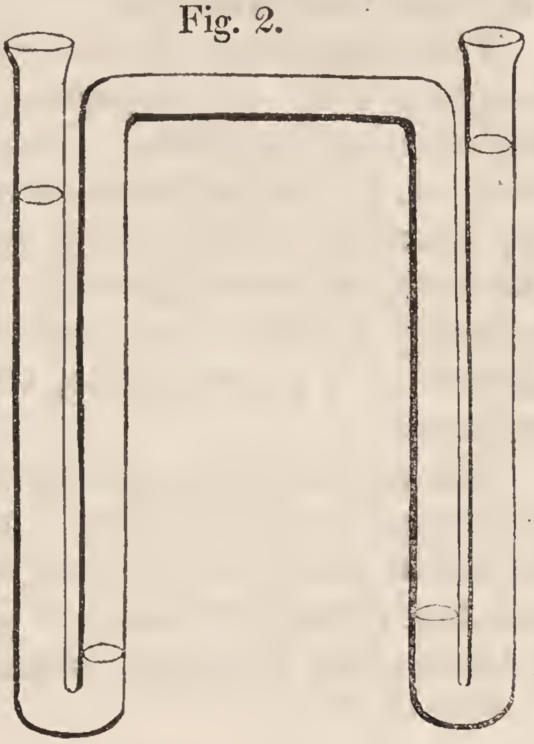
* Communicated by the Author.

† The difference of the weights of the columns of air is neglected, as of no consequence in practice.

Fahr. for instance, it must either be brought to such standard, or a small correction will be required: but every hydrometer requires a similar adjustment of far greater magnitude. In this instrument it may in general be neglected.

In the foregoing description I have, for greater simplicity, supposed the axes of the four legs to lie all in one plane; but they may be arranged differently, and on some accounts, perhaps, with advantage*. As the liquids in pouring back from the instrument will be apt to mix, this would be prevented if a small part of the upper double end of the tubes were somewhat bent, so as to stand a little above all the tubes when they are held horizontally.

Fig. 2.



By help of an instrument of this kind very wide between the middle legs (as in fig. 2.), so as to admit of each pair being put into a separate bath, the grand question regarding the temperature at which water possesses the maximum density, might be decided with greater certainty than by any other method which, so far as I know, has been employed for the purpose†. Thus if water were put into both sides, we could find at what temperature the effective column is always the shorter of the two; or, having found two temperatures at which the columns are equal, the mean between these would be very nearly the number sought. It might also be obtained by interpolating between several irregular observations lying on different sides of the maximum.

Could scales be obtained which would neither alter by moisture nor by a small change of temperature, these might be used within the baths, and the process of course would be so much the simpler. I presume, various sorts of wood, if kept dry, would undergo no change in length between 32° and 50°; and might, therefore, be safely used as scales outside the baths. An apparatus might easily be contrived, which, being placed outside,

* That part for instance containing the one liquid may be put on one side of a scale, and the other part on the other side of the same scale. This was the first form which I adopted. It is less liable to mix the liquids.

† The method employed by MM. Dulong and Petit for determining the expansion of mercury, might, I have no doubt, be employed in solving this question; but I presume the double syphon will be more easily managed.

outside, could measure both columns before it had time to change its temperature; but the other is surely sufficiently accurate.

Experiments of this kind, I intended to have made during the cold weather, but did not get them attended to, and the season is now too far advanced for conveniently obtaining the requisite temperatures.

The expansions of the columns or the difference in their lengths, may be increased or rendered more sensible by lengthening the tubes. But care must be taken to keep each bath at the same temperature throughout its whole depth. By placing a pretty wide tube open at both ends upright in the bath, and moving a solid piston in it, the temperature may be easily rendered uniform from top to bottom. Indeed, the motion of a piston alone, without a tube, would probably be sufficient.

The method proposed by Mr. Oswald Sym*, though quoted with approbation by Dr. Thomson†, is entirely a deception. It can at best give the temperature answering to the apparent greatest density of water in glass; just as if the water were in a bottle, and its height measured in the neck.

April 15, 1826. HENRY MEIKLE.

XXIV. *On the magnetizing Power of the more refrangible Solar Rays. By Mrs.* M. SOMERVILLE‡.

IN the year 1813, Professor Morichini, of Rome, discovered that steel exposed to the violet rays of the solar spectrum becomes magnetic. His experiments were repeated by Professor Configliachi, at Pavia, and also by Mons. Berard, at Montpellier, without success. I am not aware of any one having attempted them in this country, perhaps from the belief that experiments which had sometimes failed in Italy, were not likely to succeed in our more northern climate. The unusual clearness of the weather last summer, however, induced me to try what could be accomplished in this country. Accordingly, in the month of July, an equiangular prism of flint glass, the three sides of which were each 1·4 by 1·1 inch, was fixed in a slit made to receive it in a window-shutter: by this prism a coloured spectrum was thrown on an opposite panel, at the distance of about five feet. I used for the subject of experiment a very slender sewing-needle an inch

* Annals of Philosophy, vol. ix. page 387.

† System of Chemistry, 6th edition, vol. i. page 43.

‡ From the Philosophical Transactions for 1826, Part I. This paper was communicated to the Royal Society, Feb. 2, 1826.

long,

long, having previously ascertained that it was quite free from magnetism, by repeated exposure of both ends of it to the north and south pole of a very sensible magnetic needle, when it was found equally to attract either pole in every instance. The magnetic needle employed as a test in this experiment, is made of a sewing-needle magnetized, and run through a small piece of cork, into which a conical cap of glass is inserted; the whole traverses on the point of a needle fixed perpendicularly in a stand.

I had no information at this time of the manner in which Professor Morichini had conducted his experiments; but it occurred to me that it was not likely if the whole of the needle were equally exposed to the violet rays, the same influence should, at the same time, produce a south pole at one end of it, and a north pole at the other. I therefore covered half of the needle with paper, and fixed it to the panel with wax, between ten and eleven in the morning, in such a position that the uncovered part of it should be exposed to the violet rays. The needle was placed in a vertical plane, nearly perpendicular to the magnetic meridian, and inclined to the horizon. As I had not a heliostat, it was necessary to move the needle in a direction parallel to itself, to keep the exposed portion of it constantly in the violet ray.

The sun was bright at the time, and in less than two hours I had the gratification to find that the end of the needle which had been exposed to the violet rays attracted the south pole of the magnetic needle, and repelled the north pole. It had been previously ascertained that there was no iron near to disturb the results. The experiment was also repeated on the same day, under precisely similar circumstances, with the view of detecting any source of error that might have escaped observation in a first attempt; but the result was the same as in the first.

The season was so favourable that it afforded me daily opportunity of repeating the experiments, varying the size of the needles, always taking especial care to ascertain that they were free from magnetism. The needles were placed in various directions in the plane of the magnetic meridian, sometimes in the angle of the dip, sometimes perpendicular to the magnetic meridian, and also at various angles with regard to it. In some cases the heads of the needles were exposed in place of the points, to the violet rays. Perhaps it might have been expected that the influence would have been greater in those instances in which the needles were placed in the plane of the magnetic meridian, and at the angle of the dip; and, consequently, polarity might have been expected to take place in a

 shorter

shorter time under these circumstances; yet in fact, there seemed to be no difference; most of the needles became magnetic, some in longer, others in shorter periods, varying from about half an hour to four hours, but depending on circumstances which I have not yet been able to detect, further than that a number of results induced me to believe, that the experiments were more successful from ten to twelve, or one o'clock, than later in the day. The portion of the needle exposed was almost always a north pole, whether it pointed upwards or downwards: in a few instances in which the contrary occurred, it may possibly have arisen from some previous disposition in the needle to magnetism, too slight to be observed.

The distance of the needle from the prism was frequently varied by fixing the needle to the wooden pole of a fire-screen, but without material variation in the effect. I found it unnecessary to darken the room; it was sufficient to place the prism so as to throw the spectrum on any place out of the sun's rays.

My next object was to endeavour to ascertain whether any other of the more refrangible rays had the same property as the violet. A set of needles carefully examined as before, were therefore subjected to the different rays of the solar spectrum: the needles exposed to blue and green rays, sometimes acquired the magnetic property, though less frequently, and requiring longer exposure than when the violet rays were used; but the magnetism seemed to be equally strong in these as in the examples of the violet rays. The part exposed became a north pole. The indigo rays succeeded almost as well as the violet.

Pieces of clock and watch-spring were next tried, under the idea that they might, possibly from their blue colour, be more susceptible of magnetic influence,—and it was the case; their greater extent of surface, however, or their softness, may have contributed to this susceptibility. The pieces of spring were from two to three inches long, and from the eighth to the half of an inch broad. It was difficult to procure watch and clock-spring free from magnetism; it even happened on one occasion, that although the roll of spring was neutral, the pieces into which it was cut became magnetic; in one instance the pieces were heated so as to deprive them of magnetism and colour; they then required longer exposure to the rays, in order to acquire polarity. Large bodkins were exposed to the violet rays without effect, the mass perhaps being too great. When needles already magnetic were tried, their magnetism was increased. Dr. Wollaston was so kind as to lend me a very large lens, having its centre covered with paper, which he had used in his investigations respecting the chemical rays.

The

The lens concentrated the violet rays, and produced a magnetic effect in a shorter time than the prism; but the rapid motion of the sun made it difficult to keep the needle in the focus. The effect was produced with equal facility by throwing the spectrum on the floor of the room; but success could not always be depended upon even when the weather seemed most favourable.

I now made the following experiments with blue glass:—Three needles free from magnetism, having one half covered with paper, were laid horizontally on the stone outside of a south window, under a dark blue glass coloured by cobalt, in a very hot sun; after remaining in this position between three and four hours, they were found to have become feebly magnetic; the uncovered part being a north pole. On examining these needles the following day, they had lost their magnetism, a circumstance which had not before occurred, though it was observed sometimes to take place afterwards, as the force of the sun diminished from the advance of the season. There was no iron near, and the magnetic needle when placed on various parts of the stone, showed no magnetic influence in it. Next day the experiment was repeated with this difference, that the needles were left exposed to the sun, under the blue glass, six hours; and then the needles had not only acquired very sensible magnetism, but still retain it, at the distance of nearly six months. Pieces of clock-spring, which had been heated as formerly mentioned, also became magnetic under the blue glass.

I was desirous of ascertaining whether this kind of glass suffered the chemical rays to pass, and thereby occasion these changes in the steel, therefore I employed a liquid holding muriate of silver in suspension, as a test, in the following manner: A piece of writing paper dipped in the liquid was cut into two equal parts, of which one was placed under the blue glass, and the other under a white glass, as nearly at the same time as possible; but the one did not become black sooner than the other; nor on comparing them could any difference be perceived in intensity of colour, both having been equally exposed to the chemical rays. The experiment was repeated with the same result.

On the 26th of August, the thermometer at noon being 66°, two neutral pieces of clock-spring were exposed to the sun, one under a thicker piece of the same blue glass, as in the former experiment, and the other under green glass; both acquired polarity.

31st of August, the thermometer at noon 68°. Having thus

succeeded in producing magnetism under the circumstances described; I next tried the effect of exposing neutral pieces of clock-spring to the sun, enveloped in violet and green silk. The half of each was covered with paper as before, and the pieces of spring then wrapped, one in green, and the other in violet-coloured ribbon, were fixed to the inside of a pane of glass in a window, where they were left exposed to the sun all day; in the evening both had become magnetic, although they were two of the pieces of spring already said to have acquired polarity more slowly, from having been heated; and as before, the parts exposed to the sun under the ribbon were north poles.

To learn if heat had any share in producing magnetism in this case, I exposed three pieces of the same steel to a bright sunshine, on the 1st of September, the thermometer at noon being at 70°: one half of each was covered with paper, but the other half had neither glass nor ribbon over it; and although the heat was greater than on the preceding day, no magnetism was produced.

On the 2d of September, thermometer at noon 68°; a piece of neutral white steel acquired polarity from exposure to the sun, enveloped in green ribbon, one half being covered with paper as before.

On the 3rd of September, thermometer at noon 68°; two pieces of neutral spring became magnetic, one exposed in a violet-coloured ribbon, and the other in blue glass; while a similar piece of spring was in no way affected by exposure to white light: the half of each was covered with paper.

September the 4th, thermometer at noon 68°; five large sewing-needles, two inches long, were exposed to the sun's rays, one in blue glass, one in green glass, one in violet ribbon, one in green ribbon, and one in white light; the half of each was covered with paper. Of all these, two only became magnetic; namely, those in the blue glass, and in the violet ribbon.

On the 20th of September, thermometer at 69°; I placed pieces of steel enveloped in violet and green ribbon, and under glass of various colours, in different positions with regard to the magnetic meridian and dip. Several acquired polarity, the uncovered part being the north pole. A piece of steel became more strongly magnetic than usual, exposed in green ribbon, the position of which had been perpendicular to the horizon, and nearly in the magnetic meridian. For some time I still obtained similar results, though the magnetism became more feeble as the season advanced, from the diminished force of the sun; in consequence of which further experiments were deferred

deferred till the return of summer shall afford me an opportunity of continuing them.

From the results which have been stated, I am induced to believe that the more refrangible rays of the solar spectrum have a magnetic influence even in this country.

XXV. *On the Inflammation of Gunpowder by Electricity. By Mr.* Thomas Howldy.

To the Editor of the Philosophical Magazine and Journal.

Sir,

THROUGH the medium of your very useful Magazine and Journal, your ingenious correspondent Mr. Sturgeon has communicated to the public, some observations and experiments concerning the ignition of gunpowder by the charge of a Leyden jar. But as Mr. Sturgeon's method of performing the experiment is not so certain in its effect, nor more simple than Cavallo's, and as a better than Cavallo's* does not seem to have been yet made public, you will, perhaps, sir, favour me by communicating to the cultivators of electrical science, a method which I contrived more than twelve years ago, of inflaming with ease and certainty, either loose or confined gunpowder by electricity; especially as it saves the experimenter time, labour, and power.

By describing the manner in which the original experiment was conducted, the method will immediately be understood, and may be readily practised by any electrician. A jar, containing about 160 square inches of (interior) coated surface, was placed at the prime conductor, and the points of the wires of the universal discharger were set upon the table of that instrument, at the distance of one inch and a quarter from each other. A chain, which was laid upon the bare surface of the table supporting the machine and apparatus, had one of its extremities placed at the distance of four inches from the bottom or outside of the jar; while its other extremity was annexed to the negative end of the universal discharger. By this arrangement, two interruptions were made in the electrical circuit; the first or that between the points of the wires

* See his Elements of Natural or Experimental Philosophy, vol. iii. page 411. It may be here stated, likewise, that both Mr. Tatum and Mr. Lewthwaite, in their lectures on electricity delivered at the London Mechanics' Institution, had recourse to Cavallo's "water tube," as it is called by the reporter of the lectures, in order to effect the experiment in question. See the London Mechanics' Register, vol. i. p. 84, and vol. ii. p. 36.

wires was, as usual, intended to receive the substance which was to be subjected to the action of the charge; and the second, or that between the end of the chain and the outside of the jar, was intended to diminish the intensity of the charge, so as to *prevent the electrical explosion* from occurring in the first interruption. A little heap of gunpowder was then laid at the point of each wire, so as to surround and cover it; a small train of the same being laid to connect the heaps.

A moderate charge was then transmitted through the circuit, and the gunpowder was instantly inflamed by its transmission.

Having repeated the experiment several times, the distances in the interruptions being the same, the desired effect was always produced with the same intensity of the charge; and in pursuing the subject further, it was likewise found that the experiment succeeded when the distances were varied within certain limits, unless the intensity of the charge was considerably too great or too small. When the charge, in a few instances, was too intense, on its transmission a spark was seen darting between the two points, which disturbed, in a small degree, some of the gunpowder, without inflaming it; but when, in the subsequent experiment, the charge was less intense, its transmission caused the immediate inflammation of the gunpowder. Hitherto the gunpowder had been invariably disposed of in the interruption, as described in the original experiment; but I now wished to ascertain whether it would be inflamed when placed in a *single heap in any part* of the interruption, every other part being free from it. On trial it was discovered that whether the gunpowder was placed in a heap at either the positive or negative wire, or in the middle of the interruption, or in any other part of it, the passage of the charge, through the interruption, always inflamed it, though in each instance the rest of the interruption was entirely free from gunpowder.

During the performance of the preceding and many other experiments of the kind, which it is unnecessary to detail, it was observed on several occasions, that only a small portion of the charge was transmitted through the circuit, and consequently that a considerable portion of it remained in the jar; and yet the gunpowder in such cases was always inflamed. This circumstance led me to infer that the charge of a smaller jar might be successfully employed for the experiment. In consequence of which a jar whose coated surface measured half a square foot was placed at the conductor, and the transmission of its charge inflamed the gunpowder as readily as that

that of the larger jar, the whole circuit being thirty-one feet nine inches. A similar effect was produced by the charge of a phial containing forty-seven square inches of coating, the extent of the circuit remaining the same as in the above experiment. A phial containing only twenty-eight square inches of coating was next employed, and its charge when transmitted, produced the immediate inflammation of the gunpowder, the whole circuit being seven feet nine inches. This is the smallest phial in my possession, but there is no doubt that the experiment might be effected, by this method, with the charge of a phial still smaller.

After practising for several years the above simple and efficacious method of performing an experiment which has so much embarrassed electricians, another method was suggested by considering the following well-known fact; namely, that a very imperfect conductor of the electric fluid, if it has a *sharp point* and is not too extended, will convey the electricity from the prime conductor, or even from the ball of a charged jar, when presented to either of them, almost as rapidly as a metallic point. It was therefore concluded that if a sharp-pointed piece of wood was substituted for one of the pointed wires, it would convey a portion of the charge adequate to produce the intended effect. Accordingly, a small piece of very dry wood was taken, about three inches long, and a part of it was formed into a very tapering point. This was attached to the negative wire of the universal discharger, in such a manner that the wooden point projected beyond the end of the metallic point one inch and a half. The wooden point and the metallic point of the *positive wire* were then placed at the distance of half an inch from each other, and some gunpowder was laid in the interval between them. The extremity of the chain, instead of being placed so as to make a second interruption, as in the former method, was now put *in contact* with the outside of the jar. A pretty strong charge was then communicated to the jar; and as soon as the ball of the discharging rod touched that of the jar, the gunpowder was inflamed, and *nearly* at the same instant the residue of the charge passed through the flame of the gunpowder with a *smart explosion.* This unexpected and curious phænomenon very much surprised me; and the experiment was repeated several times, in order to ascertain the certainty of the fact; and with a view to render it more evident, the points were placed further from each other, and by carefully noticing and adjusting the intensity of the charge to the distance between the points, the inflammation of the gunpowder, when the *first portion* of the charge was transmitted, was seen to take place

a *per-*

a *perceptible time* before the explosion of the residual charge was heard.

As the electric explosions in these experiments do not occur till the gunpowder is in a state of combustion, it appears evident that either the product of the combustion, or the rarefaction of the air by it, is the cause of this interesting phænomenon. For the conducting power of the interval is so augmented, as to enable a *portion* of the charge having a lower intensity, to pass with explosion over a distance which the *whole charge*, having even a higher intensity, was incapable of passing over, in the same time either with or without explosion. This mode of inflaming gunpowder is related principally on account of the interesting fact which was discovered in thus conducting the experiment; for it does not possess the facility of the former, because it requires a nicer adjustment of the distance forming the interruption with respect to the intensity of the charge; and the wooden point is generally so much burned after two or three experiments have been made with it, as to be rendered useless; but as different ways of producing the same effect are sometimes desirable and pleasing as well as instructive, it may be recommended to the student in electricity who wishes to become well acquainted with the varieties of electrical action.

The latest experiments which I have made to ascertain any fact relative to the inflammation of gunpowder by electricity, were made during the severe frost which occurred here last winter. Previous to leaving the room in which the machine and apparatus are kept and employed, one night during the frost a saucer was placed upon the window-sill, and filled with pure water, which was there left to be frozen. On the following morning before any fire was lighted in the apartment, the saucer, then containing a very hard mass of ice, was put in the electrical circuit, and the interruption in which the gunpowder was placed, was made upon the surface of the ice. And when the charge of the *small phial* before described was transmitted through the circuit, the gunpowder was immediately inflamed. The experiment was repeated, under similar circumstances, the next morning, and the same effect ensued. A small Fahrenheit's thermometer denoted the temperature of the room, when these experiments were made, to be 30°.

Mr. Sturgeon supposes that the velocity with which the electric fluid moves, when the discharge passes through the circuit with explosion, is the cause of the non-ignition of the gunpowder placed in the circuit.

"Hence," says he, "my first object now was to devise some means of retarding the velocity of the electric fluid; for I con-

I considered that if this could be accomplished, *more time* would be afforded for the fluid and gunpowder *to be in contact*, and the latter, in consequence, more likely to be ignited." But this is not the case; the electric fluid does not come at all into contact with the gunpowder when it is not ignited by the explosive discharge. For this substance is a non-conductor of electricity, and the scattering and dispersing of it is not caused by any direct impetus or action of the electric fluid itself on the gunpowder, but by the rapid expansion and displacement of the air, which is driven in every direction with considerable force by the electric fluid from the point whence it explodes. This is, I believe, the true solution of the difficulty which has been so long experienced of inflaming loose or slightly confined gunpowder by the explosive discharge; and it is confirmed by the circumstance, that if a portion of any quantity of gunpowder be bruised and mixed with the rest, and the whole be well and closely confined so as to exclude the air as much as possible, an explosive charge of moderate strength on its transmission through the circuit will be found to inflame the gunpowder.

I have pleasure in perceiving that Mr. Sturgeon has realized an experiment which I had long contemplated, but had not time and opportunity to execute,—the discharge of guns by electricity; and I hope he will be successful in his attempt to render the decomposition of water by the same agent more easily practicable. I am, sir,

Your obliged servant,

Hereford, Aug. 7, 1826. THOMAS HOWLDY.

XXV. *Atmospheric Refraction at very low Temperatures and Altitudes.* *By* J. Ivory, *Esq. A.M. F.R.S.**

THE 42nd Number of the Quarterly Journal of Science, published in July last, contains a great number of refractions observed at small altitudes and very low temperatures. Such observations are of great value, and throw considerable light upon some important questions relating to the tables in the hands of astronomers. We may inquire; 1st, Whether any table hitherto published represents the refractions within 2° or 1°½ of the horizon with tolerable regularity and certainty. 2dly, Whether the tables, which, at altitudes above 2° or 1°½, are known to approach near the truth at the usual temperatures, continue to preserve the same degree of accuracy,

* Communicated by the Author.

 or

or become much more erroneous, at the unusually low temperatures in the Quarterly Journal.

1. With respect to the first question, it is already well known that all the tables entirely fail when the altitude is below 2°. But in the Quarterly Journal it seems to be insinuated that Dr. Brinkley's table has a great superiority in this respect. The following comparison of five solar refractions at altitudes less than 1°, with the quantities calculated by Dr. Brinkley's table, will enable us to judge of this point.

App. Alt.	Thermom. Fahr.	Barom.	Obs. Refr.	Brinkley.
0° 8′ 40″	−28°	30·11	81′ 19″	48′ 42″
18 40	−30	30·37	56 19	44 45
28 22	−28	30·11	54 22	40 49
39 00	−30	30·37	41 20	38 38
50 2	−28	30·11	39 47	35 11

Here it can hardly be said that the calculated refractions approach in any degree to the observed quantities. I next take all the sidereal refractions at very low altitudes. They are seven in number, and are contained in the following table:

App. Alt.	Thermom. Fahr.	Barom.	Obs. Refr.	Brinkley.
0° 29′ 25″	−28°	30·00	40′ 29″	40′ 21″
30 55	−20	30·60	42 8	39 18
43 14	−28	30·00	36 25	36 40
45 33	−20	30·60	37 23	35 41
50 53	−22	29·83	38 8	33 45
58 22	−20	30·60	33 26	32 51
58 49	−28	30·00	32 4	33 7

In three instances in the table the differences between the observed and the calculated refractions cannot be considered as excessive in the circumstances of the observations: but in the other four instances the differences are 170″, 102″, 263″, and 63″. It does not appear, therefore, that Dr. Brinkley's table can be followed as a very sure guide at such low altitudes.

We may therefore conclude that no table of refractions hitherto published can safely be trusted to at altitudes less than 2°. And this conclusion is corroborated by the parallel instance of the terrestrial refractions, which are found to vary from

from a certain limit through all degrees of magnitude, and even to change from positive to negative. It seems to be of little avail to talk of a mean value, when there occur such excessive variations depending upon circumstances mostly hidden from our knowledge. Such being the fact, we may inquire whether it can be satisfactorily accounted for. Now the refractive power of air depends upon its density, or upon its pressure and temperature which determine its density; and all experimenters have uniformly found that the distribution of heat in the stratum of air in contact with the earth is extremely irregular. Sometimes the same degree of heat which is observed at the earth's surface continues to prevail without variation to a great height; at other times, contrary to what usually happens, the heat increases as we ascend; and even when the heat decreases in ascending, the rate at which it will vary cannot be predicted with any degree of certainty. As far as the refractions are affected by the stratum of air subject to such vicissitudes, they must be irregular, as observation proves them to be. But above this first stratum of air, the vicinity of the earth has little influence on the propagation of heat, which is distributed by laws depending alone upon the nature of the aërial fluid. The atmosphere now assumes a more regular constitution; and the refractions, at elevations which exempt them from the inequalities in the stratum at the earth's surface, become subject to calculation founded on a few elements. The boundary which separates the refractions irreducible to any degree of regularity, from those that can be theoretically computed, at least with tolerable accuracy, must, it is evident, be ascertained experimentally; and it seems to be placed at an elevation of 2° or 1°½ above the horizon.

2. We are next to examine whether the tables in the hands of astronomers have nearly the same accuracy at very low degrees of heat which they possess within the usual range of temperature. And here I shall confine myself to Dr. Brinkley's table, and the table published in the Phil. Transactions for 1824, and there called the New Table. In such arithmetical examinations the head must follow the fingers; and it is easy, by a well-devised collection of instances and a little management, to establish any conclusion wished for; on which account I have included in the following table all the observations between 2° and 10°, without omitting one.

App. Alt.	Therm. Fahr.	Barom.	Obs. Refr.	New Table.	Brinkley.
2° 35′ 31″	−13°	30·26	18′ 1″	19′ 1″	19′ 18″
2 54 56	−24	30·65	18 20	18 25	18 41
3 7 34	−24	30·65	17 26	17 34	17 47
3 16 35	−13	30·26	15 33	16 16	16 24
3 19 16	−39	29·73	16 12	17 3	17 16
3 20 46	−24	30·65	17 16	16 44	16 55
3 21 9	−23	29·82	15 41	16 15	16 22
3 41 1	−39	29·73	15 27	15 46	15 52
3 49 44	−45	29·32	15 2	15 25	15 30
4 0 19	−45	29·32	14 56	14 52	14 57
4 7 20	−39	29·73	13 58	14 29	14 34
4 9 8	−45	29·32	14 43	14 28	14 32
4 10 56	−13	30·26	13 24	13 32	13 35
4 13 36	−39	29·73	13 38	13 58	14 2
4 19 42	−45	29·32	14 13	13 59	14 2
4 21 21	−41	29·76	13 52	13 56	14 1
4 21 59	−37	29·82	13 34	13 48	13 51
4 22 23	−23	29·82	12 55	13 15	13 17
4 22 50	−13	30·26	12 48	13 4	13 6
4 22 54	−39	29·73	13 47	13 47	13 48
4 22 55	−21	29·84	13 1	13 9	13 12
4 23 5	−23	29·50	13 9	13 5	13 7
4 23 8	−20	29·67	13 00	13 3	13 5
4 23 9	−27	29·95	13 16	13 25	13 28
4 23 31	−43	30·20	14 10	14 8	14 12
4 36 23	−29	29·81	13 9	12 54	12 57
4 39 31	−31	29·74	13 5	12 50	12 52
7 35 18	−27	29·80	8 36	8 18	8 17
7 37 8	−32	29·78	8 35	8 23	8 21

The inspection of this table shows that the New Table deviates less from observation than Dr. Brinkley's table in twenty-two instances out of twenty-nine; it is more distant from the truth in six instances; and in the remaining instance neither table has the superiority over the other. It cannot therefore be considered as a thing proved, although it has been said, that for altitudes less than 10°, and at very low altitudes, Dr. Brinkley's table is nearer to the truth than the New Table.

Sept. 12, 1826. J. Ivory.

XXVI. *On*

XXVI. *On the Strength of Bone.* *By* B. Bevan, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN the lists published by various authors on the cohesive strength of materials, the strength of bone has been much underrated. In the *Encyclopædia Britannica* it is quoted from Muschenbroek's experiments to be 5,250 pounds per square inch; and I have observed this number copied into various other books. But from experiments I have made with considerable care, I find the strength much to exceed that now stated, if the bone is sound and solid. There will always be some variation in the results of experiments of this nature, depending upon the quality of the substance, and also upon the mode of conducting the experiment. I have tried the bones of horses, oxen, and sheep, and find the strength of cohesion per square inch to vary from 33,000 pounds to 42,500. One specimen of fresh mutton bone supported a load in proportion to 40,000 pounds to the square inch for a considerable time, without any visible injury to the bone,—*or nearly eight times the strength given by Muschenbroek.* The modulus of elasticity of beef bone I have found 2,320,000 pounds, and specific gravity of 2·08. A substance like bone, so universally abounding, possessing such great strength, and considerable flexibility, ought to be restored to its proper place in the scale of bodies, applicable to so many purposes in the arts.

Emerson has given the ratio of the strength of bone to ash as 22 to $8\frac{1}{2}$. Now by direct force applied to ash, its cohesion appears to be 16,000 pounds to the square inch, making bone upon the ratio above stated to be more than 41,000 pounds, the correctness of which has been confirmed by my experiments.

I have been trying the cohesive force of various species of wood, and find them generally to exceed the strength given by Mr. Barlow in his treatise on the strength of timber.

My apparatus is partly like that used by Mr. Barlow on the direct cohesion; but the mode of applying the load, instead of adding large weights, is that of gradually advancing a given weight along the arm of a lever, allowing the weight to rest occasionally for several minutes, and sometimes for three or four hours. By this machine I found that a good clear-grained specimen of English oak supported a load in proportion to 19,800 pounds to the square inch, for several hours, having left it at seven in the evening with that load suspended without

out any visible alteration at 10 o'clock, but at 6 o'clock the following morning a cylindrical pin had been drawn from the large end of the specimen without separating the fibres. We may therefore safely allow 20,000 pounds for the strength of this oak.

In a similar manner a pin was drawn out of a specimen of Spanish mahogany, with a load in proportion to 22,000 pounds per square inch, and ash with a load of 16,000 per square inch. It will be observed that my results considerably exceed those of Mr. Barlow; the dimensions of my specimens were larger than those of Mr. Barlow, and conducted with great care, and with much greater weights to produce fracture. Mr. Barlow's mode of taking the dimensions of his specimens I consider liable to objection, for the silk thread used to measure the circumference of the cylinders would give the diameter too large by the diameter of the thread; and this quantity would be of considerable moment in affecting the area of the cylinders. I shall probably take an early opportunity of sending you the results of my experiments on the cohesion of various species of wood. Remaining yours truly,

B. Bevan.

P. S. If you think the following dimensions of a gourd worth notice, it is at your service. Now growing at Goldington in the county of Bedford, in the garden of Mr. Addington, a gourd 5 feet 5 inches in circumference, full 11 inches high, supposed to weigh 70 pounds, not at present done growing,—is 8 feet from the root of the vine.

XXVII. *Instructions for collecting Geological Specimens.* *By* William Henry Fitton, *M.D. F.R.S. V.P.G.S.**

It so often happens that specimens sent from distant places, by persons unpractised in geology, fail to give the instruction which is intended, from the want of attention to a few necessary precautions, that the following directions may perhaps be useful to some of those, into whose hands these pages are likely to fall. It will be sufficient to premise, that two of the principal objects of geological inquiry, are, to determine,—1st, the nature of the *materials* of which the earth is composed; and, 2ndly, the relative *order* in which these materials are disposed with respect to each other.

1. Specimens of rocks ought not, in general, to be taken

* From the Appendix to Captain P. P. King's "Narrative of a Survey of the Inter-tropical and Western Coasts of Australia."

from

from loose pieces, but from large masses in their native place, or which have recently fallen from their natural situation.

2. The specimens should consist of the stone unchanged by exposure to the elements, which sometimes alters the characters to a considerable distance from the surface. Petrifactions, however, are often best distinguishable in masses somewhat decomposed; and are thus even rendered visible, in many cases, where no trace of any organized body can be discerned in the recent fracture.

3. The specimens ought not to be too small.—A convenient size is about three inches square, and about three-quarters of an inch, or less, in thickness.

4. It seldom happens that large masses, even of the same kind of rock, are uniform throughout any considerable space; so that the general character is collected, by geologists who examine rocks in their native places, from the average of an extensive surface:—a collection ought therefore to furnish specimens of the most characteristic varieties;—and *the most splendid specimens are, in general, not the most instructive.* Where several specimens are taken from the same place, a series of numbers should be added to the note of their locality.

5. One of the most advantageous situations for obtaining specimens and examining the relations of rocks is in the sections afforded by cliffs on the sea-shore: especially after recent falls of large masses. It commonly happens that the beds thus exposed are more or less inclined; and in this case, if any of them be inaccessible at a particular point, the decline of the strata will frequently enable the collector to supply himself with the specimens he wishes for, within a short distance. Thus, in the subjoined sketch, which may be supposed to represent a cliff of considerable height,—the observer being situated at *a*, the beds *b*, *c*, *d*, though inaccessible at that place, may be examined with ease and security, where they successively come down to the shore at *b'*, *c'*, and *d'*.

6. To examine the *interior* of an unknown country, more skill and practice are required: the rocks being generally concealed by the soil, accumulations of sand, gravel, &c., and by the vegetation of the surface. But the strata are commonly disclosed

disclosed in the sides of ravines,—in the beds of rivers and mountain-streams; and these, especially where they cross the direction of the strata, may be made, by careful examination, to afford instructive sections.

7. Among the occasional components of the strata, the remains of organized bodies,—shells, corals, and other zoophytes,—the bones and teeth of animals,—fossil wood, and the impressions of vegetable stems, roots, or leaves, &c., are of the greatest importance; affording generally the most marked characters of the beds in which they occur.—These should, therefore, be particularly sought after, and their relative abundance or rarity in different situations noticed. The petrified bodies should, if possible, be kept united with portions of the rock or matrix in which they are found; and where they are numerous,—in sand, clay, or any moist or friable matrix,—it is in general better to retain a large portion of the whole mass, to be examined afterwards, than to attempt their separation at the time of collecting.

8. The *loose materials* which are found above the solid rocks, in the form of gravel, silt, rolled pebbles, &c., should be carefully distinguished from the *solid strata* upon which they rest. And the more ancient of these loose materials, found on the sides or summits of hills, &c., should be distinguished from the recent mud, sand, and gravel, brought down by land-floods or by rivers. The bones and teeth of animals are not unfrequently found in the more ancient gravel; and the collection of these remains from distant quarters of the globe, is an object of the greatest interest to geology.

9. Besides a note of the locality, there ought, if possible, to accompany every specimen, a short notice of its geological circumstances; as—

Whether it be found in large shapeless masses, or in strata?

If in strata,—what are the thickness, inclination to the horizon, and direction with respect to the compass, of the beds?—[If these cannot be measured, an estimate should always be recorded, while the objects are in view.]—Are they uniform in dip and direction?—curved, or contorted?—continuous, or interrupted by fissures or veins?

Is the whole cliff, or mass of strata in sight of uniform composition?—or does it consist of different kinds of stone?

If the strata be different,—what is the order in which they are placed above each other successively?

10. A label, distinctly written, should accompany every specimen, stating its native place, its relative situation, &c. &c. And these labels should be connected with the specimens immediately,

mediately, on the spot where they are found*.—This injunction may appear to be superfluous; but so much valuable information has been lost to geology from the neglect of it, that every observer of experience will acknowledge its necessity; and it is, perhaps, in practice one of the most difficult to adhere to.

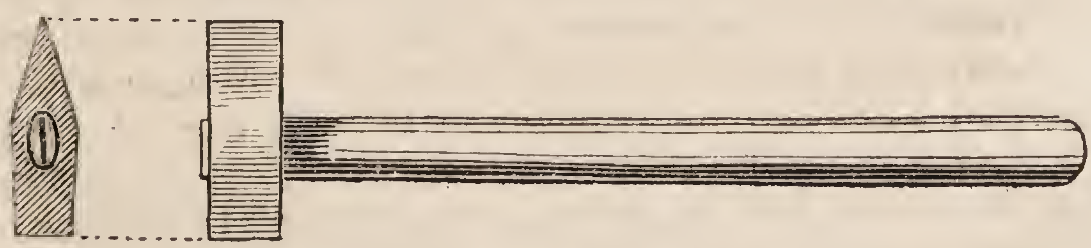
11. A sketch of a coast or cliff, however slight, frequently conveys more information respecting the disposition and relations of rocks, than a long memorandum. If numbers, denoting the situation of the specimens collected, be marked upon such sketches, much time may be saved at the moment of collecting. But in all such cases, the memorandum should be looked over soon afterwards, and labels distinctly explaining their situation, &c., be attached to the specimens themselves.

12. The specimens should be so packed, that the surfaces may be defended from exposure to air, moisture, and friction; for which purpose, if strong paper cannot be obtained, dry moss, or straw, or leaves, may be employed. Where paper is used for wrapping the specimens, they are best secured by fastening the envelope with sealing-wax.

Lastly, The collector must not be discouraged, nor be prevented from collecting, by finding that the place which he may chance to visit in a remote situation, has not a striking appearance, or the rocks within his view a very interesting character; since it frequently, and even commonly, happens, that facts and specimens, in themselves of very little importance, become valuable by subsequent comparison; so that scarcely any observation, if recorded with accuracy, will be thrown away.

The Instruments required by the geological traveller will vary, according to the acquirements and specific objects of the individual. The most essential are:—

The hammer; which, for general purposes, may be of the form here represented:—

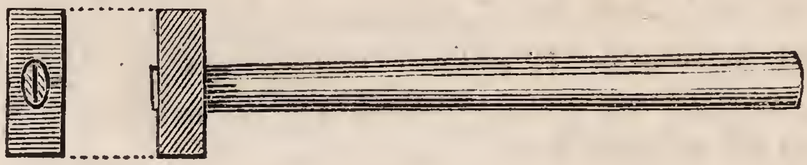


The head should be of steel well tempered, about 4 inches from the face to the edge, and $1\frac{1}{4}$ inch square in the middle;

* It is useful to mark on the labels the day, and even the hour, when each specimen is collected. This, with a corresponding note in the memorandum-book, will be found to assist the memory, and prevent confusion.

the face flat, and square, or nearly so; the edge placed in the direction of the handle. The orifice for the insertion of the handle oval, a very little wider on the outer side than within; its diameters, about 1 inch vertically, and 7-10ths across; the centre somewhat more than $1\frac{1}{2}$ inch from the face. The handle should be of ash, or other tough wood; not less than 16 inches long; fitting tight into the head at its insertion, without a shoulder; and increasing a little in size towards the end remote from the head, to prevent its slipping.—It should be fixed in the head by means of a thin, barbed iron wedge.

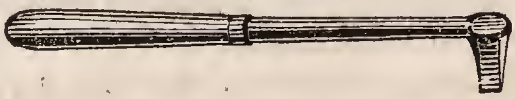
For trimming specimens, smaller hammers may be employed:—The form of the head, recommended for this purpose by Dr. MacCulloch*, is rectangular. The dimensions of the face may be 1 inch by $\frac{3}{4}$; the height $2\frac{1}{4}$.



It will be expedient to have always some hammers, (or at least the heads,) of different sizes, in reserve.

A small miner's pick is useful for cutting out, and splitting portions of slaty rocks; or for obtaining specimens of clays, &c.

A small stone-cutter's chisel.—A chisel with a handle, of the form here represented, will often save the hand of an inexpert collector, and better enable him to direct his blow.



For packing the specimens.—A stock of strong paper; sealing-wax; writing-paper, cut into labels; thick gum-water, to cement the labels to the specimens.

For the conveyance of specimens.—A large bag of leather, with straps for the shoulders. Strong canvas bags, of smaller size, are very convenient for subdivision and arrangement.—For the protection of crystals, or delicate petrifactions, &c., wool or cotton are necessary; and small wooden boxes (like those used for holding wafers) are sometimes required. For distant carriage, strong wooden boxes, casks, or baskets.

The following are either essential, or useful in various degrees, for obtaining and recording observations.

Pocket memorandum-books, of sufficient size to admit

* "On the forms of Mineralogical Hammers." Quarterly Journal, (R. Inst.) vol. xi. 1821, p. 1, &c.

sketches;

sketches; a pocket compass; a measuring-tape, of fifty feet or more; a telescope; a camera lucida; a box of colours.

The best maps should always be sought for:—and, the true œconomy to the traveller being that which saves time, it is best to mark, or even to colour the map, in the field. Notes inserted on imperfect maps, or deduced afterwards from memoranda, are less authentic; and the process is frequently neglected.

Portable barometers, with detached thermometers, are desirable; and the best instruments are ultimately the cheapest. But, unfortunately, barometers of every construction are very easily damaged or deranged.—Minute accuracy, however, in the determination of heights, though very interesting to physical geography, is comparatively of little importance to the geologist.

If the collector be a surveyor, he will know best to what purposes a pocket sextant, or a small theodolite, is applicable: —the measurement of distances,—of heights,—and of the inclination of strata, &c.

XXVIII. *Notice of the volcanic Character of the Island of Hawaii, in a Letter to Professor* SILLIMAN, *and of various Facts connected with late Observations of the Christian Missionaries in that Country, abstracted from a Journal of a Tour around Hawaii, the largest of the Sandwich Islands**.

THE island of Owhyhee, now called Hawaii, has long been famous as the scene of the death of the celebrated English navigator Captain James Cook.

The atrocity of that scene, although extenuated by some circumstances of provocation, contributed to stamp the character of the natives with the charge of extreme barbarity; a charge which seems, however, to have had no peculiar foundation; the character of the Owhyheans appearing to be substantially the same with that of all the inhabitants of the Polynesian groups.

However this fact may have been, an effort was thought worth making to elevate this interesting people to the condition of civilized and Christian men. It is well known, that in October 1819, a mission sailed from Boston for the Sandwich Islands, where they arrived in April 1820; and that an additional number of missionaries sailed from New-Haven† in November 1822, and arrived in April 1823.

Soon

* From Silliman's Journal, vol. xi. p. 1.

† Among these missionaries was Mr. Joseph Goodrich, who, while a member

Soon after the arrival of this second missionary family, a tour round the island was resolved upon, with particular reference to the great objects of the mission. Messrs. Ellis *, Harwood, Thurston, Stewart, Bishop, and Goodrich, were charged with the execution of this duty, which they performed with zeal and ability. The result of their observations is detailed in a little volume, ably drawn up by Mr. Ellis, and entitled "A Journal of a Tour around Hawaii, the largest of the Sandwich Islands." Besides many interesting statements relative to the paramount objects of the enterprise, it contains a great number more relating to the natural history of the island. From this part of the work, we intend to quote the most important passages; and we conceive that we cannot better introduce them than by the following letter from Mr. Goodrich to the Editor, which, although dated a year ago, has been received only within a few days.

Letter from Mr. Joseph Goodrich, one of the American Missionaries in the Sandwich Islands.

To Professor Silliman, New-Haven, (Connecticut).

My dear Sir, Waiakea (Hawaii), April 20, 1825.

I confess I have remained silent quite too long, in not answering your kind request on the eve of my embarkation, although I am better able to state facts now than at any former period. The station which I am called to occupy, is on the N.E. side of Hawaii, (pronounced Harwye,) at the head of a safe and commodious harbour, yet but little known to foreigners. About forty miles in the interior, in a south-westerly direction, is a burning volcano, that has been in a state of activity from time immemorial. The oldest natives can give no account of a time when it was not burning: they say it is more active now than it was twelve or fifteen years since †.

On

member of Yale College, applied himself with diligence to the study of mineralogy and geology, with particular reference to more extended usefulness as a missionary, he having already resolved on devoting himself to that object.

Mr. Goodrich made very considerable acquirements in this way; and being endowed with a vigorous frame, and peculiar hardihood and equanimity, he was well qualified for the vicissitudes of a missionary life in a barbarous country.

The letter annexed, contains so many interesting notices, that I have given it with little abridgement or alteration. Mr. Thurston and Mr. Whitney were also from Yale College, and possessed, in a high degree, the requisite traits of character.

* An English missionary then on a visit at Hawaii.

† There is now a whaling ship in this port, the Dawn, of New-York, Captain Butler, seven and a half months out, which will probably return

about

On my arrival at these islands, I landed at Oahu, and spent two or three months there. The rocks that I examined there are decidedly volcanic, yet many bear a near resemblance to the trap rock. The soil, in many places, is quite red, and is used by some for red paint; and for any thing that I know, answers every purpose of Spanish brown. The soil is the same on Tuuai (Atooi). From what I have seen and heard of all the islands, there is no doubt, in my mind, that they are all volcanic.

The summer after my arrival, I spent about ten weeks in making a tour of this island, in company with several other members of the mission family. A journal of that tour will probably be published in America. The island of Hawaii, from the north point to the southern, including all the west side of the island, is little else than one entire mass or sheet of lava, which has run down from the mountains at different periods. Some of the currents of lava are so recent, that there is no vegetation to be seen upon them; but others are of a much more ancient date, so that bushes and even trees have sprung up among the beds of lava. Most of the land on the western side of the island four or five miles from the shore is high, probably not far from 3000 feet above the level of the sea. In several places, the lava, as it ran down from the mountains, fell over precipices from 20 to 100 feet in height; sometimes presenting the form of stalactites, and at others of stalagmites, and sometimes an entire sheet, like the falling of water over a mill dam, except that the lava was more viscid.

The most remarkable place is eight or ten miles to the south of Kearakekua, which place is to the southward of the middle of the island. There are four high mountains in the island, one back of Toaehae, and another back of Kairua, upwards of 7000 feet high, called Hualulae: the two others are vastly higher; namely, Mouna Kea, to the northward and eastward part of the island, estimated to be upwards of 18,000 feet high, and Mouna Roa, in the south-western part, probably near the same height. I have been twice to the summit of Mouna Kea. The first time I was at the highest peak about three o'clock at night, in the month of August; the thermometer

about two years hence. Capt. B. expects to come here next fall, and likewise the spring following, on board of whom it is my expectation to put a box of minerals for you, unless I have an opportunity short of that time. I might now send it down to Oahu, the port from which almost all vessels clear out for America. Should I send it down there, it would be uncertain in what vessel they would be shipped, or at what port in America they would arrive. It would then be altogether uncertain whether you would ever receive them. I think it preferable to wait for a good opportunity.

meter stood at 27 deg. 5 below the freezing point. I passed over several banks of snow, that lay to the northward of the highest peaks, (this mountain rises much more abruptly than Mouna Roa,) and the change was so great in passing from a torrid to a frigid zone, that I was under the necessity of travelling all the time I was up there to prevent freezing. The second time that I ascended was in April last. There appear to be three or four different regions in passing from the seashore to the summit. The first occupies five or six miles, where cultivation is carried on, in a degree, and might be to almost any extent; but as yet, not one twentieth part is cultivated. The next is a sandy region, that is impassable, except in a few foot-paths. Brakes, a species of fern, here grow to the size of trees; the bodies of some of them are eighteen inches in diameter. The woody region extends between ten and twenty miles in width. The region higher up produces grass, principally of the bent kind. Strawberries, raspberries, as large as butternuts, and whortleberries flourish in this region, and herds of wild cattle are seen grazing. It is entirely broken up by hills and valleys, composed of lava, with a very shallow soil. The upper region is composed of lava in almost every form, from huge rocks to volcanic sand of the coarser kind. Some of the peaks are composed of coarse sand, and others of loose stones and pebbles. I found a few specimens that I should not hesitate to pronounce fragments of granite. I also found fragments of lava, bearing a near resemblance to a geode, filled with green crystals, which I suppose to be augite.

Very near to the summit, upon one of the peaks I found eight or ten dead sheep; they probably fled up there to seek a refuge from the wild dogs; I have heard that there are many wild dogs, sheep and goats. Dogs and goats I have never seen.

I was upon the summit about 2 o'clock P.M., the wind S.W., much resembling the cold blustering winds of March with you, the air being so rare that it produced a severe pain in my head, that left me as I descended. Much more might be said, that I must omit for want of room. The volcano that I before mentioned is by far the greatest curiosity in the islands. I presume that it is the largest known; at least it is by far the largest of any of whose dimensions I have seen an account. I have made four visits to the volcano. The last time, I measured the circumference with a line, and found it to be seven and a half miles. Some part of the way I measured within the crater, where the wall was 300 or 400 feet above us. I counted twelve different places where lava was red hot, and three

three or four where it was spouting up lava thirty or forty feet. The depth of the crater is probably above 1000 feet; down about 500 feet is a black ledge, which appears to have been formed by the crater's being filled up with lava one half way, and the lava being discharged by an outlet under ground. The crater appears to be filling up, for when I was there the last time, I perceived that the lava had run 30 or 40 feet over a place where I crossed the bottom when I was up there about six weeks previous. The lava was then so hot that I could only cross the edges, where it had run out. In the middle of this place it was still spouting out lava. I crossed the bottom in several places that looked quite smooth, as viewed from the top; but on descending I found the surface to be made up of hills and valleys. Dense sulphurous fumes are ascending from almost all parts of the bottom; some of the gaseous substances appeared to smell like muriatic acid gas: the gases are very suffocating, so much so that the crater is impassable in many places. In many places, the escaping of the gaseous substances makes a tremendous roaring, like the steam let out of the boiler of a steam-engine. On the night of the 22d of December 1824, a new volcano broke out at the bottom of the large crater; as soon as it was sufficiently light, I descended near to the spot where the lava was both spouting up and boiling like a fountain; some of the lava was thrown forty or fifty feet into the air. It was one of the most awful scenes that I ever witnessed, to see such a mass of lava, red hot, boiling and running like water, although it was not so liquid as water: by sun-rise it had run fifty or sixty rods, and eight or ten rods wide. As I was alone, standing within a few rods of the running lava, I heard a crashing among the rocks of lava behind me. I judged it prudent to retrace my steps. On my visit there six weeks after, I found that it had formed a mound of the lava that had issued out, upwards of sixty feet above the bottom of the crater. The black ledge that I mentioned above, extends all round the crater except a few yards; it forms a kind of stair, although it is half a mile wide some part of the way. The crater upon this ledge measures five and a half miles in circumference. Capillary volcanic glass is in great abundance in some places upon the bottom, to the depth of two or three inches; and some is to be seen fifteen or twenty miles from the crater, drifted by the wind and lodged in the crevices of the lava. There are also great quantities of pumice stone about the crater, but so very light and porous, that it is driven about by every puff of wind. It is so delicate in its texture that it is very difficult to preserve the specimens. Fifteen or twenty miles in a southerly direction, the steam and vapours

vapours are issuing through almost the whole distance from the cracks and fissures of the lava. The form of the crater is something of the shape of an egg, the longest diameter from N. to S. When one is in the crater, and viewing the rocks below the black body, (which is covered with very porous volcanic glass,) lava of all descriptions may be seen, from that which is loose and porous to that which is very firm and equally compact as any of the trap rocks. From what I have seen since I have been upon these islands, I should not hesitate to class lava and trap rocks together; for how can a part of the same mass be in a state of fusion and part not? That which appears to have been under the greatest pressure, is uniformly the most compact. I shall endeavour to send you specimens the first opportunity, although they will not be large, in consequence of having so far to carry them by hand. The land about the crater has fallen in, including a space not much short of six miles in diameter. To the north end of the crater, the land is nearly level for a considerable distance, then it gradually descends to the sea-shore: the volcano is probably 8000 or 10,000 feet above the level of the sea: the ground or rocks are also full of cracks and fissures, that render it rather dangerous travelling. When I was up there in December, a native fell through the grass and rubbish into one of the fissures that was concealed, and was drawn up by a rope much bruised.

There are large quantities of sulphur in and about the crater, where, also, whortleberries are growing all the year, but they are not so palatable as those in America; they are about the size of red cherries; the natives do not eat them, considering them sacred to the god of the volcano. There is also a plenty of wild geese, though not so large as tame geese. The lava in many places is full of the crystals of augite and leucite. The sand upon the sea-shore in front of my house is composed chiefly of green crystals, which I suppose to be augite. I have tried several specimens of the lava, and find them fusible by the blow-pipe. For further information I must refer you to a journal of a tour of this island that was made the summer after my arrival.

We will now proceed to give, as far as the object in view is concerned, an abstract and analysis of the tour of the missionaries, as drawn up by Mr. Ellis; and although some of the facts are the same as those related by Mr. Goodrich, they are presented in such a connexion, that it will not be unpleasant or unprofitable to have them in part repeated. Mr. Goodrich's letter contains, however, a number of facts not related in the tour. "The tour," says The North American, (for April 1826,) "was begun at Kairua, a village on the western side of

of the island, and the residence of Kuakini, the principal chief of Hawaii. They proceeded along the coast to the south, east, and north, till they had encompassed the island, having occupied in their rambling a little more than two months. They made frequent excursions inland, visited the principal villages, conversed with the people, preached to them on proper occasions, and collected such information as in the most satisfactory manner to answer the ends of the mission. A guide was furnished them, called Makoa, a person of a somewhat remarkable appearance and character, to judge from his picture, and the description of him in the book. But he was faithful to his duty, and the travellers were hospitably received, and civilly treated wherever they went."

In the report of the deputation, which is prefixed to the narrative of Mr. Ellis, they remark: "We have been enabled to collect considerable information on a variety of subjects, which, though of secondary moment, in the missionaries' account, are nevertheless interesting and important; such as the natural scenery, productions, geology and curiosities; the traditionary legends, superstitions, manners, customs," &c. "In the prosecution of our design, to *explore* and *enlighten* the long benighted Hawaii, we have ascended its lofty and majestic mountains, entered its dark caverns, crossed its deep ravines, and traversed its immense fields of rugged lava. We have stood with wonder on the edge of its ancient craters, walked tremblingly along the brink of its smoking chasms, gazed with admiration on its raging fires, and witnessed with no ordinary feelings of awe, the varied and sublime phænomena of volcanic action, in all its imposing magnificence and terrific grandeur."

The Hawaiians, we are assured, like other barbarous nations, are accustomed to recognise "the presence of some unpropitious deity"—"in the sighing of the breeze, the gloom of the night, the boding eclipse, the meteor's glance, the lightning's flash, the thunder's roar, and the earthquake's shock."

They have a goddess of volcanoes, whom they call *Pele*, and "they are continually reminded of her power, by almost every object that meets the eye, from the rude cliffs of lava, against which the billows of the ocean dash, even to the lofty craters, her ancient seat amid perpetual snows."

The volcanic character of Hawaii is highly interesting, and the proofs of this character presented by the missionaries are so numerous, that they recur almost every where in their progress, and so satisfactory, that their statements cannot fail to produce entire conviction.

In the vicinity of Kairua, they attempted to excavate a well,

as there was no good water within five or six miles of the town. In the prosecution of this effort, "they entered several caverns in the lava, resembling an arched vault, or extended tunnel of various thicknesses and dimensions. They supposed the lava at the edges of the torrent had first cooled, hardened, and formed the side walls, which approximated as they rose, until uniting at the top, they became a solid arch, inclosing a stream of lava, which continued to flow on towards the sea. One of these tunnels, called Raniakea, they found to be of considerable extent. After entering it by a small aperture, they passed on, in a direction nearly parallel with the surface, sometimes along a spacious arched way, not less than twenty-five feet high and twenty wide; at other times by a passage so narrow that they could with difficulty press through, till they had proceeded about 1200 feet. Here their progress was arrested by a pool of water, of considerable extent and depth, and salt as that found in the hollows of the lava within a few yards of the sea. This latter circumstance, in a great degree damped their hopes of finding fresh water, by digging through the lava. In their descent, they were accompanied by more than thirty natives, most of whom carried torches. On arriving at the water, they simultaneously plunged in, extending their torches with one hand, and swimming about with the other. "The partially illuminated heads of the natives, splashing about in this subterranean lake, the reflection of the torch-light on its agitated surface, the frowning sides and lofty arch of the black vault, hung with lava, that had cooled in every imaginable shape, the deep gloom of the cavern beyond the water, the hollow sound of their footsteps, and the varied reverberations of their voices, produced a singular effect; and it would have required little aid from the fancy, to have imagined a resemblance between this scene and the fabled Stygian lake of the poets."—" The mouth of the cave is about half a mile from the sea, and the perpendicular depth to the water, is probably not less than fifty or sixty feet. The pool is occasionally visited by the natives, for the purpose of bathing, as its water is cool and refreshing. From its ebbing and flowing, it has probably a direct communication with the sea." It was ascertained that the point which forms the northern boundary of the bay, and "runs three or four miles into the sea, is composed entirely of lava, and was formed by an eruption from one of the large craters, on the top of Mouna Huararai, about twenty-three years ago, which filled up an extensive bay, twenty miles in length, and formed the present coast. A number of villages, plantations, fish-ponds, &c. were at that time destroyed."

It

It was observed that in several places "the sea rushes with violence along the cavities beneath the lava, to a considerable distance, and then, forcing its waters through the apertures in the surface, forms a number of *jets d'eau*, which falling again on the rocks, roll rapidly back to the ocean."

In the morning of June 28, 1823, "Messrs. Thurston, Goodrich, and Harwood, walked towards the mountains, to visit the high and cultivated parts of the district. After travelling over the lava for about a mile, the hollows in the rocks began to be filled with a light brown soil; and about half a mile further, the surface was actually covered with a rich mould, formed by decayed vegetation and decomposed lava." The fences were made with the fragments of lava, inclosing small and well cultivated fields, "planted with bananas, sweet potatoes, mountain tard, tapa trees, melons, and sugar-cane, flourishing luxuriantly in every direction."

After passing three or four miles, through this "delightful region," they found several pools of fresh water, and arrived at the woody region, which extends several miles up the sides of the lofty mountain, that rises immediately behind Kairua.

This mountain, called Kuararai, is one of the three highest in Hawaii, and it was very natural that the travellers should wish to ascend to its summit. "The varied strongly marked volcanic surface" of the higher parts—"the traditional accounts of its eruptions, the thick woods that skirt its base, and the numerous feathered tribes that inhabit them," all conspired to make it an object of high interest. About 8 o'clock, on the morning of July 9th, they commenced the ascent, accompanied by three native guides. After travelling about 12 miles, they arrived at the last house on the western side of the mountain. Their guides were unwilling to proceed any further that night, but the missionaries proceeded without them, and "travelled about six miles over a rough and difficult road, sometimes across streams of lava full of fissures and chasms, at other times through thick brush-wood, or high fern, so closely interwoven as almost to arrest their progress." They passed the night in a temporary hut, erected on the lava, and the morning of the 10th was ushered in by the singing of birds. The thermometer was 46 deg. on the outside of the hut,—on the sea-shore it is usually about 84 deg. Having united in their morning orisons, they proceeded on their journey; "their road lying through thick underwood and fern, was wet and fatiguing, for about two miles, when they arrived at an ancient stream of lava, about twenty rods wide, running in a direction nearly west. Ascending upon the hardened surface of this stream, over deep chasms, and huge volcanic stones, a distance of

 three

three or four miles, they reached the top of one of the ridges, on the western side of the mountain." They met with strawberries which were rather insipid, and with raspberries which were white and large, but not so well flavoured as those of Europe and America.

"Between nine and ten in the forenoon they arrived at a large extinguished crater, about a mile in circumference, and apparently 400 feet deep. The sides were regularly sloped, and at the bottom was a small mound, with an aperture in its top. By the side of this large crater, divided from it by a narrow ridge of volcanic rocks, was another, fifty-six feet in circumference, from which volumes of sulphurous smoke and vapour continually ascended. No bottom could be seen, and on throwing stones into it, they were heard to strike against its sides for eight seconds, but not to reach its bottom. There were two other apertures very near this, nine feet in diameter, and apparently about two hundred feet deep. Walking along its giddy verge, they could distinguish the course of two principal streams, that had issued from it in the great eruption about the year 1800. One had taken a direction nearly north-east. The other had flowed to the north-west, in broad irresistible torrents, for a distance of from 12 to 15 miles to the sea, and, driving back the waters, had extended the boundaries of the island. The party attempted to descend the great crater, but the steepness of its sides prevented their examining it so fully as they desired. After spending some time there, they walked along the ridge between three and four miles, and examined sixteen different craters, similar in their construction to the first they met with, though generally smaller in their dimensions. The whole ridge appeared little less than an assemblage of craters, which, in different ages, had deluged the valleys below with floods of lava, or showers of burning cinders. Some of them appeared to have reposed a long period, as they were covered with earth and clothed with verdure. Trees of considerable size were growing in some of them." They found a fruit resembling the whortleberry, which, although insipid, was juicy, and supplied the place of water. The party were however unable to reach the summit, as they had been a day and two nights without water and saw no prospect of procuring any. After passing another night on the lava, they therefore reluctantly returned towards Kairua. In their descent, they discovered an excellent spring of water, by which the party were much refreshed. They had travelled so constantly upon the sharp points of lava, that their shoes were nearly destroyed, and they returned almost barefoot to the governor's at Kairua. Although the attempt

to

to reach the summit of the mountain was unsuccessful, the excursion gave them the fullest evidence of the volcanic origin of this region. On the 16th of July, Messrs. Goodrich and Harwood, from the extremities of a base line of 2,230 feet, made two observations, by which they made the height of the mountain 7,882 feet; but as their quadrant was not a good one, it was concluded that the real height exceeded this. The mountain is, however, never covered with snow.

On the 18th of July they proceeded forth on their journey; and about the middle of the day, near Kahalu, they "travelled about a mile across a rugged bed of lava, which had evidently been ejected from a volcano, more recently than the vast tracts of the same substance by which it was surrounded. It also appeared to have been torn to pieces, and tossed up in the most confused manner, by some violent convulsion of the earth, at the time it was in a semi-fluid state. There was a kind of path formed across the most level part of it, by large, smooth, round stones, brought from the sea-shore, and placed three or four feet apart. By stepping from one to another of these we passed (as they remark) over the roughest pieces of lava we had yet seen."

On the 19th their way lay over a rough tract of lava, resembling that which they passed the day before.—They go on to relate:

"In many places, it seemed as if the surface of the lava had become hard, while a few inches underneath it had remained semi-fluid, and in that state had been broken up, and left in its present confused and rugged form. The rugged appearance of the lava was probably produced in part, by the expansive force of the heated air beneath the crust of lava; but this could not have caused the deep chasms and fissures which we saw in several places: we also observed many large spherical volcanic stones, the surface of which had been fused, and in some places had peeled off, like a crust or shell, an inch or two in thickness. The centre of some of these stones, which we broke, was of a dark blue colour and clayey texture, and did not appear to have been at all affected by the fire."

On the 21st of July, the travellers arrived at the spot where, in the year 1780, Tamahameha gained a decisive victory over his cousin and rival Kauikeouli, and thus laid the foundation of his power. The battle lasted eight days, and "the scene of this sanguinary engagement was a large tract of rugged lava, the whole superficies of which had been broken up by an earthquake."

On the 24th, near Keakoa, a singular appearance of the lava attracted the attention of the party. "It consisted of a covered

covered avenue of considerable extent, from 50 to 60 feet in height, formed by the lava's having flowed, in some recent eruption, over the edge of a perpendicular stratum of very ancient lava, from 60 to 70 feet high. It appeared as if it had at first flowed over in one vast sheet, but had afterwards fallen more slowly, and in detached semi-fluid masses. These, cooling as they fell, had hardened and formed a pile, which, by continued augmentation from above, had ultimately reached the top, and united with the liquid lava there. It was evident that the lava still continued to flow along the outside of the arch thus formed, into the plain below, as we observed, in several places, the course of unbroken streams, from the top of the cliff, to the bed of smooth lava, that covered the beach for several miles. The space at the bottom, between the ancient rocks and more recently formed lava, was from six to twelve feet. On the one side, the lava rose perpendicular and smooth, showing distinctly the variously coloured strata of which it was composed; some of a bright scarlet, others brown and purple. The whole mass appeared to have undergone, since its formation, the effects of violent heat. The cracks and hollows, horizontally between the different strata, or obliquely through them, were filled with lava, of a florid red colour, and much less porous than the general mass. It must have been brought to a state of most perfect liquefaction, as it had filled up every crevice that was more than half an inch wide. It appeared highly glazed, and in some places we could discover small round pebbles, from the size of a hazel-nut to that of a hen's egg, of the same colour, and having the same polish, yet seeming to have remained solid, while the liquid lava, with which they were mixed, had been forced by subterranean fire, into all the fissures of the ancient rock.

The pile on the other side, formed by the dripping of the lava from the upper edge of the rocks, presented a striking contrast, but not a less interesting scene. It was generally of a dark purple or jet black colour, glittering in the rays of the sun as if glazed over with a beautiful vitreous varnish. On breaking any fragments of it, we found them very porous, and considerably lighter than the ancient lava, on the other side. Its varied forms baffled description, and were equal to the conceptions of the most fertile imagination. The archway thus formed, extended for about half a mile, occasionally interrupted by an opening in the pile of lava, caused by some projecting rock, or elevation in the precipice above. A spectacle awfully sublime and terrific must have been presented, when this burning stream rolled in one wide sheet, a fiery cascade, from the lofty steep, down upon the smoking plain. With what

what consternation and horror must it have filled the afflicted inhabitants of the surrounding villages, as they beheld its irresistible and devastating course, impressed as they were, with the belief that Pele, the goddess whom they had offended, had left her abode in the volcanoes, and was in person visiting them with thunder, lightning, earthquake, and liquid fire, the instruments of her power and vengeance. As we passed along this vaulted avenue, called by the natives Keanaee, we beheld a number of caverns and tunnels, from some of which, streams of lava had flowed. The mouths of others being walled up with stones, we supposed were used as sepulchres. Mats spread upon the slabs of lava, calabashes, &c., indicated some of them to be the habitations of men; others, near the openings, were used as workshops, where women were weaving mats, or beating cloth. Some we also saw used as store-houses, or depositories of sandal-wood. In many places, the water filtered through the lava, and around the spots where it had dropped upon the ground, we observed a quantity of very fine, white, spear-shaped crystals, of a sharp nitrous taste. Having walked a considerable distance along the covered way, and collected as many specimens of the lava as we could conveniently carry, we returned to the sea-shore, the path along which was often tedious and difficult, The lava frequently presented a mural front, from 60 to 100 feet in height, in many places hanging over our heads, apparently every moment ready to fall; while beneath us the long rolling billows of the Pacific chaffed and foamed among the huge fragments, along which our road lay. In many places, the lava had flowed in vast torrents over the top of the precipice into the sea. Broad flakes of it, or masses like stalactites, hung from the projecting edge in every direction. The attention was also attracted by a number of apertures in the face of the rocks, at different distances from their base, looking like so many glazed tunnels, from which streams of lava had gushed out, and fallen into the ocean below, probably at the same time that it had rolled down in a horrid cataract from the rocks above.

On the 25th, Messrs. Thurston, Goodrich and Bishop continued their journey along the shore, which was "literally iron-bound." It was formed of steep rocks of lava, whose surface wore the most rugged aspect imaginable. About 2 P.M. they passed Taureonahoa, three large pillars of lava, about 20 feet square, and 80 or 100 high, standing in the water within a few yards of each other, and adjacent to the shore. Two of them were united at the top, but open at their base. The various coloured strata of black, reddish and brown lava being

being distinctly marked, looked like so many distinct courses of masonry."

After leaving Kalahiti, they "proceeded over a very rugged tract of lava, broken up in the wildest confusion, apparently by an earthquake, when it was in a semi-fluid state. About noon they passed a large crater. Its rim, on the side towards the sea, was broken down, and the streams of lava issuing thence, marked the place by which its contents were principally discharged. The lava was not so porous as that of Keanaee; but, like much in the immediate vicinity of the craters, was of a dark red, or brown ferruginous colour, and but partially glazed over." For a mile along the coast, they found it impossible to travel without making a considerable circuit inland, and they continued to pursue their way over a broken and rugged tract of lava.

In this volcanic country, the want of fresh water is severely felt, and was often experienced by the missionaries during their tour.

On the 26th, at Kapua, they hired a man to go about seven miles into the mountains, for fresh water; but he returned with only one calabash full, a very inadequate supply for the party, who had suffered much from thirst, and the effects of brackish water. They now entered the district of Kau, and turning the southern point of the island, found "the same gloomy and cheerless desert of rugged lava, spreading itself in every direction, from the shore to the mountains. Here and there at distant intervals they passed a lonely house, or a few wandering fishermen's huts, with a solitary shrub of thistles struggling for existence among the crevices in the blocks of scoria and lava: all besides was one vast desert, dreary, black and wild. Often all traces of a path entirely disappeared. For miles together, they clambered over huge pieces of vitreous scoria, or rugged piles of lava, which like several of the tracts they had passed in Kana, had been tossed into its present confusion by some violent convulsion of the earth."

Their narrative proceeds: "From the state of the lava, covering that part of the country, through which we have passed, we should be induced to think, that eruptions and earthquakes had been almost without exception, concomitants of each other; and the shocks must have been exceedingly violent, to have torn the lava to pieces and shaken it up in such distorted forms, as we every where beheld." "Slabs of lava, from nine to twelve inches thick, and from four to twenty or thirty feet in diameter, were frequently piled up edgewise, or stood leaning against several others, piled up in a similar manner."

manner." "Some of them were six, ten, or twelve feet above the general surface, fixed in the lava below, which appeared to have flowed round their base, and filled up the interstices occasioned by the separation of the different pieces. One side of these rugged slabs generally presented a compact, smooth, glazed, and gently undulated surface, while the other appeared rugged and broken, as if torn with violence from the viscid mass, to which it had tenaciously adhered. Probably these slabs were raised by the expansive force of heated air, or of steam, beneath the sheet of lava." "A number of conical hills, from 150 to 200 feet high, rose immediately in our rear, much resembling sand-hills in their appearance. On examination, however, we found them composed of volcanic ashes and scoria; but could not discover any mark of their ever having been craters." So common were streams and masses of lava wherever the missionaries travelled, that upon this harsh substance they almost always walked, sat, and slept. On the evening of the 26th, they spread their "mats upon the lava, and lay down to sleep under the canopy of heaven." A pile of blocks, of scoria, and lava; part of which they had themselves erected, screened their heads from the wind.

The company were much distressed for want of water, but were relieved by the natives, who appear to have been uniformly kind and hospitable.

After leaving Keavaiti on the 27th, "Messrs. Thurston, Bishop, and Goodrich travelled over the rugged lava, till the moon becoming obscured by dark heavy clouds, they were obliged to halt under a high rock of lava, and wait the dawn of day; for they found it impossible to proceed in the dark, without being every moment in danger of stumbling over the sharp projections of the rocks, or falling into some of the deep and wide fissures that intersected the lava in every direction. During the whole of the 27th, a most beautiful spouting of the water attracted the attention of the travellers. It was produced by the rolling of the waves of the sea, which, through an aperture of about two feet in diameter, every few seconds threw up a volume of water with considerable noise, and a pleasing effect, to the height of thirty or forty feet. The lava at this place was very ancient, and much heavier than that at Kona. The vesicles were completely filled with olivin.

Almost every incident connected with this interesting tour is in some way associated with lava. Villages and funeral piles, and sanctuaries of refuge were built upon lava. This substance was often the missionary's pulpit, when he preached, and the seat of the people while they heard the tidings of salvation; and at night, the pilgrims often sought repose upon

 this

this rough and pointed bed. Incessant almost as was its recurrence, it often presented something new or more striking than what had appeared before. On the 30th they travelled over another tract of lava "about 200 rods wide, which had been violently torn to pieces, and thrown up in the wildest confusion. In some places it was heaped forty or fifty feet high. The road across it was formed of large, smooth, round stones, placed in a line two or three feet apart." On these the missionaries passed over, by stepping from one to another, but not without considerable fatigue. They were shown the place where, in one of the wars of Tamehamaha, a party of his enemies, about 80 men, being the warriors of two villages, were, during their repose at night, destroyed by a volcanic eruption.

In their progress along the south-eastern side of the island, they arrived at the village of Milone, celebrated on account of a short pebbly beach called Shoroa. Of these stones they had been accustomed to form, not only cutting instruments, but to fabricate gods. It required considerable skill to select those which would answer; and as they were supposed to be endowed with sex, one of each kind was selected, when they were about to be transformed into gods. They were wrapped up together in a piece of cloth, and after a certain time a small stone was found with them, which, when grown to the size of its parents, was taken to the heiau and made afterwards to preside at the games.

Although the climate of Hawaii is hot, and the thermometer on the evening of July 31st stood at 70°, the air from the mountains soon became so keen that, although in a tropical climate, they found a fire very comfortable.

As they were travelling upon the high land, they perceived a number of columns of smoke and vapour rising at a considerable distance, and also one large steady column that seemed little affected by the wind, and which, as they were told, arose from the great crater of Kirauea.

The next day three of the party visited the places where they had seen the columns of smoke rising the day before.

They travelled five miles over a considerably fertile and cultivated country, the soil of which was composed of the decomposed surface of a bed of ancient lava, upon which shrubs and trees had grown to a considerable height. As they approached the places from which the smoke issued, they passed over a number of fissures or chasms, from two inches to six feet in width. "The whole mass of rocks had evidently been rent by some violent convulsion of the earth, at no very distant period," and when they came in sight of the ascending columns of

of smoke and vapour, they beheld, immediately below, a valley or hollow, about half a mile across, formed by the sinking down of the whole surface of ancient lava to the depth of fifty feet below its original level. It was intersected by narrow fissures, running in every direction, and two ran from the mountain towards the sea, as far as the eye could reach. From the wider portions of these fissures, where they were about ten or twelve feet in width, the smoke arose. As they descended into the valley the ground sounded hollow, the lava cracked under their feet, and soon grew (as they proceeded) so hot that they could not stand more than a minute or two in a place.

Their guide, terrified by the smoke and vapour that issued from one of the apertures, refused to go any further, remonstrating against the audacity of the strangers, who presumed thus to provoke the anger of the goddess Pele, the local deity of the volcano, although the guide retreated to the bushes at the edge of the valley, while the travellers proceeded. They passed as near as the smoke and sulphureous vapours would permit, to several of the fissures. Although they looked into several, it was only in three that they could see any bottom. These appeared to be about 50 or 60 feet deep, and contained red hot stones that had fallen in; and they thought they saw flames, but the smoke and heat were so great, that it was difficult to look long. Their hands, legs, and faces were nearly scorched by the heat.

They walked along the hollow for nearly a mile, and arrived at a chasm from which lava had very recently issued, both in projected fragments and in streams.

"The appearance of the tufts of long grass, through which it had run; the scorched leaves still remaining on one side of a tree, while the other was reduced to charcoal; and the strings of lava hanging from some of the branches like stalactites; together with the fresh appearance of the shrubs, partially overflowed and broken down, convinced them that the lava had been thrown out only a few days before. It was of a different kind from the ancient bed of which the whole valley was composed, being of a jet black colour and bright variegated lustre, brittle and porous; while the ancient lava was of a gray or reddish colour, compact, and broken with difficulty."

The heat varied at the surface, which they attributed to the varying thickness of the lava, beneath the whole of which, the heat was still in great activity, as was evinced by the volumes of smoke and vapour every where issuing. Of this place Mr. Ellis took a drawing.

It appeared from the statement of the guide, that about

eleven

eleven moons ago, the two large chasms were formed, and that the great hollow had been formed by the subsidence of the earth, about two moons ago, in consequence of an earthquake. The missionaries regarded this as an infant volcano, which seems, however, to have remained mainly undisturbed for a long time, perhaps for ages; for "the lava is decomposed to a considerable depth, and is mingled with prolific soil, fertile in vegetation, and profitable to its proprietors." We felt, they observe—"a melancholy interest in witnessing the first exhibitions of returning action, after so long a repose in this mighty agent, whose irresistible energies will probably, at no very distant period, spread desolation over a district, now smiling in verdure, repaying the toils and gladdening the heart of the industrious cultivator." The place which the missionaries had visited, is about 10 or 12 miles from the sea shore, and about 20 from the great volcano, at the foot of Mouna Roa.

As they returned, they "passed several hills, whose broad base and irregular tops, showed them originally to have been craters. They must have been very ancient, as they were covered with shrubs and trees. From them must have come the then molten, but now indurated floods, over which the party had been travelling."

Having made every preparation to visit the great crater of Kirauea, the party set forward at 5 P.M. of July 31.

At a place called Kapuahi, they "stopped at the entrance of a large cave, arched over by a thick crust of ancient lava." This cave, although with no other light than that which entered at the mouth, was inhabited permanently by entire families—whose members were cheerfully employed in domestic industry within, while the children were playing among the fragments of lava, without. Although very poor, they imparted to the travellers both fresh water and taro root.

The progress of the party was now over a most beautiful country, which, to the right, sloped gradually for ten or fifteen miles to the ocean, and rose abruptly to the left, "where it was crowned with the woods, which extend, like a vast belt, round the base of Mouna Roa. At the distance of three or four miles they came to another cavern in the lava, called Keapuana, which is often used as a lodging place for benighted travellers. "The entrance, which was eight feet wide and five high, was formed by an arch of ancient lava. The interior of the cavern was about fifty feet square, and the arch that covered it was ten feet high. There was an aperture at the northern end, about three feet in diameter, occasioned by the falling in of the lava, which admitted a current of keen mountain air, through the whole of the night. While they were

cleaning

cleaning out the small stones between some of the blocks of lava, that lay scattered around, a large fire was kindled near the entrance, which, throwing its glimmering light on the dark volcanic sides of the cavern, and illuminating one side of the huge masses of lava, exhibited to our view the strange features of our apartment, which resembled in no small degree, scenes described in tales of romance."

From the higher regions in the vicinity of the cave, the light of the volcano illuminating the clouds, was distinctly visible.

[To be continued.]

XXX. *On an Anomalous Case of Vision with regard to Colours.* *By* George Harvey, *Esq. F.R.S. E.**

As the following anomalous case relating to the vision of colours appears to possess some remarkable peculiarities, I have considered it of sufficient importance to be submitted to the Royal Society of Edinburgh.

J. B., aged 60 years, served in early life an apprenticeship to a farmer; but disliking agricultural pursuits, became a tailor, and afterwards entered into the navy, and served in several general actions. After quitting the sea-service he resumed his trade, and in the employment of which he now continues. From his childhood, it appears, he was unable to point out colours by their proper names; or, excepting in a few cases, to distinguish one colour from another. From the nature of his avocation, this circumstance must have often been to him the source of much inconvenience; and during his whole life he has found the utmost embarrassment from it. He has remarked that his inability of distinguishing colours has cut him off from the enjoyment of many innocent and harmless pleasures. If a painting were placed before him abounding with the most beautiful varieties of colour, it would only present a dull and cloudy appearance; and hence he has never made a practice of stopping at print-shops, or of visiting any scenic representations. In early life, he once visited a panoramic exhibition, and he remarked, that his mortification was extreme, when he found every one around him delighted with the splendour of the scenes; whereas to him, to adopt his own words, the whole presented nothing but "a smoky appearance." The face of nature also, which, to the perfectly organized eye, presents so many exquisite varieties of colour, and so many

* From the Edinburgh Philosophical Transactions, vol. x. p. 253.

beautiful

beautiful diversities of light and shade, has always appeared to him under a dark and murky aspect. While others have contemplated with high gratification the splendour of the setting sun, or the glory of the rainbow, he has seen but little to admire; and, when led by the chances of a seaman's life, into the Mediterranean, where a bright sun and a pure and cloudless sky lend to the glowing tints and the vivid colouring of nature charms unknown to the climates of the North, the contrast produced no peculiar effect on him; nor has this arisen from a morbid constitution of mind; for, on the contrary, he is remarkably happy and cheerful; and, from all the information I have been able to obtain respecting him, has always been distinguished for his steadiness, cheerfulness, and good conduct.

From several opportunities that I have had of examining into the peculiarities of his case, I have drawn up the following brief observations.

Of whites, he appears to have a very good idea, and so also of grays;—he having selected five pieces of cloth of the latter colour, and arranged them according to their varieties of shade, with perfect ease. By candle-light, however, he mistook a thread of pink silk for gray, and, under the same circumstances, confounded flax-flower blue (No. 29. Werner's Nomenclature of Colours by Syme) with the same colour.

On Syme's page of blacks being presented to him, he thought the whole to be dark-green. At first he pointed out the specimen which had the darkest tone; but after a few minutes, he remarked that they all appeared the same. When specimens of basalt and hornblende were placed before him, he could perceive no difference, although the former had a grayish, and the latter a greenish hue. Between raven and fine velvet black he could perceive no difference. His master furnished me with the following fact. Being desired to repair an article of dress that required black silk, he employed crimson; and a similar mistake occurred on two other occasions.

Both indigo and prussian blue (24 and 25 of Syme) he regarded as black. China and azure blue (26, 27, *ibid.*) he considered to be blue, but thought them good matches for carmine-red (90), when placed by its side. Ultramarine blue (28), he thought to be the same as lake-red; and when a light lake-coloured wafer was laid on a piece of azure-blue cloth, he thought the resemblance very perfect. Flax-flower blue (29), he could distinguish as blue. His master informed me that he once confounded sky-blue with green when repairing some article of dress; and on another occasion, when a young gentleman's dark-blue coat was brought to him for immediate

repair,

repair, the mother of the lad was surprised to find the elbow of the coat repaired with crimson*.

Syme's purples he regarded in general as blues; the only one to which he could perfectly adapt his notion of that colour being the imperial purple (39), of a specimen of fluor-spar. To the pale blackish purple, the colour of the porcelain-jasper, he would only give the name of dark colour. A blue lilac he called a lead colour; nor could he trace a shade of purple in the sweet-scented violet, or in a plum. From all the information I could obtain, he regarded purple as a slight modification of blue.

Greens were to him a source of much embarrassment. On a particular occasion, I requested him to bring me eight or ten specimens of cloth, of different shades of that colour. This I found from his master, was the occasion of much uneasiness to him; and he at last was compelled to ask one of his fellow-workmen to point out the green bundle to him, although they had been charged not to assist him in his difficulty. His master having discovered this circumstance, substituted some pieces of black and brown for some of the greens; and he, unaware of the change, furnished the following as varieties of green.

Four specimens of dark bottle-green:

One	reddish-black	(21)	Syme.
One	raven-black	(22)	
One	liver-brown	(104)	
One	blackish-brown	(108)	

When Syme's specimen of verdigris-green was placed before him, he declined giving any name to it, but remarked that it was certainly not green. The beautiful green of the emerald (52), he called pale orange; and to grass-green he applied the same remark as to verdigris. Duck-green, which forms so interesting a feature in the neck of the mallard, he named brown or green, displaying much uncertainty; and the same ambiguity was manifested when olive-green was shown him. On another occasion, being requested to point out two colours in the page that resemble each other, he immediately fixed on the two last-mentioned, and again called them brown.

All his ideas of green appeared to be extremely confused. On being told that Syme's specimens were varieties of that colour, and requested to point out one that bore a resemblance to the green fields, he expressed his surprise at the remark,

* In one of Dr. Nicholls' cases, published in the Medico-Chirurgical Transactions, the following anomalous circumstance is recorded. "Charles was in the navy, and, several years ago, he purchased a blue uniform coat and waistcoat, with *red* breeches to match the blue."

and

and contended that they bore no resemblance to it. The darker kinds of green he considered to be brown; those of a middle tone ambiguous, and the lighter kinds, as in the case of emerald-green, of a pale orange colour.

He experienced no difficulty with any of the brighter varieties of yellow. From a number of pieces of different coloured cloth, he immediately selected a specimen of this colour, and a fragment of high-coloured sulphur he thought a beautiful example of the same. Gallstone-yellow he conceived to be orange. Wax-yellow (13), which in the vegetable kingdom exists in the greenish parts of the nonpareil apple, he supposed might be a green. His ideas of yellow were, however, on the whole very correct.

His notions of orange were very imperfect. The common marigold, he called yellow; and a sample of fine orpiment, orange; and likewise equally choice specimens of reddish and deep reddish orange he termed brown.

Of the reds, he considered carmine, lake, and crimson, to be blues;—the latter, indeed, a dark-blue, agreeing with the instance of the coat before alluded to. When a great snapdragon was placed before him, he called the margin of its upper lip, which was purplish-red, a dark colour, and thought it a very good match for my black coat. The part also near the throat of the corolla, and which was of a light blueish-red, he called light sky-blue. The yellow palate he distinguished perfectly; but as that colour gradually lost itself in the purplish-red, he gave it the name of *black*. He remarked, that, when a boy, the ruddy cheeks and arms of the milk-maidens always appeared to him of a blueish tone; and on being shown a rosy child, he persisted in the same remark. The carnation, pink, and the cock's comb presented also the same appearance. Scarlet-red he distinguished readily by its proper name. Veinous blood* he assimilated to brown or black; and the brown disk of the common marigold, and the iron-flint, although presenting so marked a difference to the perfectly formed eye, appeared to display no variety to him. To the latter, indeed, he gave the name of *olive*. The colours of the common garden rose and peach-blossom were both designated lead-colour. To him, therefore, some of the sweetest and most delicate colours of creation presented but little beauty.

In the case of browns, there was much uncertainty; in the greater variety of cases assimilating them to green. Mineral

* Mr. Dalton, in vol. v. p. 33 of the Manchester Transactions, remarks that in his own case blood appears not unlike bottle-green; and, it is worthy of notice, that in the present case, it was termed " not a black but nearly so."

pitch,

pitch, although clearly a variety of brown, he considered to be black; and liver-brown he designated in the same manner. Chesnut-brown he could not distinguish. An article of dress, indeed, of the latter colour, he repaired with silk of an olive-green; and, on another occasion, he considered covered buttons of a bottle-green, as a perfect match for a dress of a blackish-brown. Two fragments of cloth, one a duck-green, and the other a liver-brown, were placed before him, and he was unable to point out the difference.

From his having regarded crimson-red both as dark-blue and black, it was anticipated that he would confound the two latter. This accordingly took place, when the indigo and prussian blues of Syme's Nomenclature of Colours were shown him; calling them both black, notwithstanding they exhibited a marked distinction in that excellent work.

From the preceding observations it appears, that the only colours he was capable of distinguishing with certainty (by day), were white, yellow, and gray; and that in the proper perception of the following colours, there appeared to be varied degrees of uncertainty.

Colours proposed for observation.	Perceptions of the proposed colours.
Black.	Generally green, in particular cases crimson.
Blue.	Darker kinds, crimson and black. Those of a middle tone carmine and lake. Those of a lighter kind able in general to distinguish.
Purple.	Appeared to be blue, excepting in the case of a very bright purple.
Green.	Confounded in general with black and brown. Greens of a darker kind appeared brown. Those of a middle tone uncertain as to name. Greens of a lighter kind dark yellow.
Orange.	Darker kinds, brown. Lighter kinds, yellow.
Red.	Carmine, lake, and crimson, appeared blue.
Brown.	Green.

Since the above was written, the following circumstances occurred. He was sent to obtain some patterns of green baize, and having procured five varieties, placed them on his return in the shop-window. In the course of the day, his master called on him to state the prices of the patterns he had obtained, at the same time placing before him two pieces of crimson cloth, and three of the green baize; and he, unconscious of the difference, fixed prices on each.

On being informed of the circumstance, I prepared two excellent specimens of the above colours, and placed them before

 him,

him, first on a white ground; secondly, on a yellow ground; and lastly, on one of a green colour; and in each case, he regarded the two colours, to use his own words, "as well matched*."

The day being favourable on which the last experiment was performed, a vivid and well-formed solar spectrum was thrown on the wall. He pronounced it to be composed of two colours, yellow and light blue; and which, in a former conversation, he described as the ordinary appearance of the rainbow. The vivid and brilliant red of the spectrum he could by no means distinguish. Its general appearance he regarded as in some degree beautiful; but the bursts of pleasure which escaped from my children, as they contemplated the brilliant colours in succession, appeared to excite in him the greatest surprise. I afterwards found, that he considered the prism as a thing moderately interesting, but as by no means meriting the praises which had been bestowed on it.

His eyes appeared to be excellently well formed, and time has but slightly impaired their powers. According to the opinion of my friend Mr. Tracey, surgeon, they appear to possess all the essentials necessary to good sight; namely, *perfect transparency of the several humours, a proper degree of convexity of the cornea and ball of the eye*, and to which may be added, *the perfectly healthy functions of its appendages;* the proofs of which are discovered in the just adaptation of the eye to distance. In his communication to me on the subject, Mr. Tracey observes, "If I might adduce any one point (which under common circumstances, I should not be disposed to notice), I should say the gray colour of the irides is much fainter than usual; and that the pupillæ are extremely small." The cause of the last-mentioned fact, is no doubt to be attributed to the constant exercise of the eyes by candle-light, it being known, that, persons similarly occupied, are of necessity obliged to bring the object very near, and thus, from long habit, produce artificially a permanent diminution in the magnitude of the pupillæ.

In the present limited state of our information on this very interesting and curious subject, and with so few cases that have been hitherto presented to the philosopher, it may be premature to offer any very decided observations on the cause. The objects of a true and legitimate philosophy are perhaps best fulfilled, by diligently collecting facts, and, by cautiously deducing inferences from them, to form gradually the successive links of a chain, which shall ultimately lead to the true cause.

* A philosophical friend who was present, placed the colours under several different circumstances, but with the same uniformity of effect.

Of

Of the hypotheses that have been proposed to account for the phænomena in question, that which refers it to an insensibility of the retina to certain impressions of light, appears at once the most simple and philosophical. Some eyes, it is well known, are capable of performing the general functions of vision, and are yet unable to perceive those minute impressions of light, which to other eyes are readily perceptible. A retina may be perfectly adapted to receive the due and proper effect of a beam of light, and yet, from some peculiarity in its organization, incapable of perceiving all its component parts. In the present case, the general objects of vision, as relate to form, distance, and magnitude, were perfectly fulfilled, and the sensations arising from white, yellow, gray, and the lighter varieties of blue, appeared in general to be correct; and it would therefore appear probable, that the retina was sufficiently sensible to receive correct impressions from pure white, up to colour of a certain intensity; and, beyond which, its power being enfeebled, it communicated only imperfect and confused ideas*.

In the cases published by Dr. Nicholls, in the Medico-Chirurgical Transactions, the anomaly appeared in some measure hereditary; in one instance being derived through the father, and in the other through the mother. In some families this peculiarity of vision appears to be transmitted through a son, without affecting in the smallest degree a daughter; and in other cases precisely the reverse. In the present instance, however, the anomaly appears to have originated with the individual, no member of his family being similarly circumstanced, —he having always regarded himself as a singular and unfortunate exception to the whole.

Plymouth, July 20, 1823.

XXXI. *Account of the Descent of a Diving-Bell, newly invented by* T. Steele, *Esq. M.A.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

As improvements in physical science at present excite an interest in this country altogether without precedent, and as the great national work the Breakwater in Plymouth Sound, the Pearl Fisheries, the bold enterprise of searching the Spanish galleons sunk for more than a century in Vigo-Bay, the search of the Lutine frigate sunk upon the coast of Holland, &c.

* The point where this change appeared to take place, with respect to blue, was between ultramarine and flax-flower blue, corresponding to 28 and 29 of Syme.

&c. &c. have recently called more than ordinary public attention to the operations of the diving-bell,—it may be perhaps not uninteresting to you to receive an account of a very novel experiment which I have lately witnessed.

This was the descent of Mr. Steele, in a bell which, from its peculiar properties, he calls "*the Communicating Diving-Bell*," for which he has obtained a patent; as also for some apparatus above water, with which the bell is in connexion, constructed upon a theory which leads, in the application of its principle, to some exceedingly curious and interesting consequences.

The bell consists of two compartments in combination: one of them the common diving-bell, open at the bottom; but the second is a close chamber with a bottom, called by the inventor "*the Communicating Chamber.*" This compartment is supplied with air by pipes reaching above water, and is separated from the former by a partition, in which is set a small circular window of extraordinary strength and thickness, but the glass is of such clearness that not a particle of light seems to be lost in its transmission; so that the vision from one chamber to the other is just as distinct as if no such medium were interposed.

The inventor, before descending, placed a lighted lantern in the open compartment, he next entered the communicating chamber through a circular aperture, which was firmly screwed down upon him; and he then gave orders to be lowered under water.

In this descent, the water was prevented from entering the open bell by the ordinary operation of the condensing air-pump upon deck; and Mr. Steele had consequently an opportunity of observing every thing within it, strongly illuminated by the lantern; while at the same time, instead of being obliged to endure the pressure of condensed air, and to depend upon signals by strokes of a hammer, he sat at his ease in air of no greater density than that of the atmosphere, and conversed through one of the pipes with his friends above water.

While he remained down, he described what he saw in the open bell, and gave such directions as circumstances rendered necessary. At one time, for instance, he observed that the water was rising within its cavity, and he ordered that the condensing air-pump should be worked more strongly; this was accordingly done, and the water immediately descended until it was on a level with the bottom. During this time, by simply turning a cock, he had the means of refreshing the air in his chamber at pleasure, by a current of condensed air from the open bell. After some time he desired that they should "heave up,"

up," which was done: the water-tight cover was unscrewed, and he came out, evidently very much gratified by the success of his first experiment.

He next took his seat in the open bell, and a "bell-man" took the place he had just ceased to occupy. I was informed that Mr. Steele had selected this man for his steadiness, having been down with him before while he was making some experiments with a common diving-bell. The communication between persons descending in this manner is by writing: they write upon tablets, and mutually exhibit them at the window in the partition. They descended: the bell-man described what he saw to the persons above; and after they had been under water for some time, when Mr. Steele chose to ascend, he intimated his desire to him, and he immediately communicated the order that the bell should be raised. After they came out of the bell, preparations were made for an experimental proof of the theory which I have alluded to, as leading in its application to such remarkable consequences.

An air-tight chamber above water, with a window like the one already described, was connected by a flexible pipe with the open diving-bell: the bell-man took his seat in this, one of the engineer's workmen went into the communicating chamber, and Mr. Steele himself, taking his tablets and pencil, went into the chamber above water, into which he was immediately fastened by an air-tight cover. The bell was lowered under water, and of course the chamber above was filled through the pipe with condensed air.

At this moment a novel and complex system of communicating by conversation and writing was thrown into action, instead of signals by the strokes of a hammer. Mr. Steele in the air-chamber above held conversation with the bell-man below, and the man in the communicating chamber below conversed with the persons upon the deck of the vessel. A gentleman stood close to the chamber above, and he and Mr. Steele exhibited writings to each other through the window. The latter wrote among other things, that he had just asked the man below "if he was anxious to be heaved up," and that he had been answered in the negative. The man then made some observations about the noise in the bell caused by the rush of the condensed air through the valve, and about some other circumstances bearing relation to the experiment. At this time Mr. Steele required some paper; and a sheet tightly rolled was given in to him through a cylinder passing through the side of the chamber. This was furnished with two cocks, one on the inside, and the other on the outside; so that the paper was transmitted without the escape of the air. Soon after this, I observed

served that he exhibited at the glass an order for heaving up; and the bell was raised. When he was liberated, and while he was receiving the congratulations of his friends, I heard him say to one of them that "his feeling was one of extreme delight at the moment when, looking out from the little window of the air-chamber, after the full success of his experiments, he saw his diving-bell emerging slowly from the water, and the persons who had descended come out from it in safety."

There is an exceedingly interesting but apparently paradoxical consequence resulting from the theory upon which the air-chamber has been constructed: 1st, Let the diameter of the pipe between this chamber and the bell be supposed to be increased until it be sufficient to admit a man, and let a rope ladder be inclosed in it.—2nd, Let the air-chamber be subdivided into two, with a man-hole in the partition, and another in the side. It is evident that by a process analogous to that of passing locks in a canal, and identically the same with that used by Mr. Steele himself in the transmission of the roll of paper through the two air-tight cocks, that a man might go down unwet from deck, to blast rocks, or do any other work at the bottom of the sea! The same thing might be effected by the addition of a third compartment to the bell below, between the communicating chamber and the open bell; in which case, the air-chamber above water would (for this particular purpose) cease to be necessary.

It is manifest, however, that these *two modes* of passing and repassing between the deck and the bottom, might be used *even in combination.* I have the honour to be, &c.

Portsmouth, Sept. 1826. * * *.

XXXII. *On the Velocity of Sound.* *By* Wm. Galbraith, *Esq. M.A.**

THE determination of the motion of sound in an elastic medium has frequently been an object of discussion. Nothing very important relative to the mathematical theory was effected till the time of Newton. That great philosopher devoted the eighth section of the second book of his *Philosophiæ Naturalis Principia Mathematica* to the determination of motion propagated through fluids, which has served, in a great degree, for the basis of most that has been written on the subject.

In the scholium subjoined to the last proposition in that section, he enters upon the motion of sound transmitted

* Communicated by the Author.

through

through the atmosphere, and obtains a result corresponding to the most accurate experiment then known. His notions relative to the *crassitude of the solid particles of air*, and vapours floating in the atmosphere, seem rather illogical, and give his reasoning the appearance of an attempt to reconcile his theory with observation upon principles hardly admissible. In fact, the part of the *Principia* which treats of the nature and velocity of the aërial pulses is confessedly obscure, and the soundness of the reasoning has been called in question by philosophers of the greatest eminence. On this point, indeed, it is difficult to form an opinion entirely free from objection; and it is agreed on all hands, that the velocity of sound assigned by Newton is accurate if the law of elasticity on which his investigation proceeds be admitted.

If these seemingly gratuitous assumptions, to which we have already alluded, be rejected, the velocity of sound determined by experiment is greater than the quantity computed by Newton's formula by one sixth of the whole, from theory.

Laplace having considered this subject, conjectured that the true explanation of this great difference was to be found in the law of Boyle and Mariotte; namely, that the elasticity of air is directly as its density. Now this law is exact only when the temperature remains unchanged. It is admitted that a series of aërial pulses is a succession of condensations and rarefactions, which are always accompanied with the disengagement and absorption of heat, attended by a corresponding change in the elasticity of the air. The temperature is, no doubt, soon equalized by the transfer of heat between the atmosphere and the air in motion. But to produce this effect, a sensible time is required; while the elastic force is exerted instantaneously, the agitated air still retaining all its heat of combination. From these remarks it appears that the Boylean law ought not to be adopted in the investigation of the velocity of sound, without some modification. It is obvious that the elastic force of air, which changes its volume while it retains the whole of its absolute heat, ought to be employed. When this explanation is attended to, the difference between theory and experiment is so small that it may safely be ascribed to unavoidable errors of observation.

The question now to be determined, is, how far this principle will affect the velocity of sound. Messrs. Clement and Desormes by experiment have determined the *constant**, which we

* This quantity is said to be constant; but as its determination by very accurate experimenters varies considerably, it may, it is conjectured, undergo a slight modification according to circumstances. The French academicians

we shall call k, to be 1·3492, and Gay-Lussac and Welter 1·3748. The mean of these two is 1·362, by which the result must be multiplied before the root is extracted in the formula of Newton. If the law of Boyle and Mariotte be adopted, then $\frac{p}{p'} = \frac{s}{s'}$, in which p is the elastic force of the air in agitation, p' that *in equilibrio;* and s and s' the corresponding densities. This equation leads to the formula of Newton, that is, $v = \sqrt{l \times g}$, in which l is the height of the homogeneous atmosphere, and g the gravitating force. But if the constant $k = 1{\cdot}362$ be introduced, then $v = \sqrt{l \times g \times k}$ ... (1)

Now if p be the pressure or height of the mercury in the barometer, and D the density of a homogeneous elastic medium, then Newton's formula becomes $v = \sqrt{\frac{pg}{D}}$. Biot has determined the ratio of the weight of mercury to air at the temperature of the freezing point, and under a pressure of 29·921 inches in the parallel of 45°, to be 10466·82. But when the barometric pressure varies and becomes p, and the temperature t by the law of Boyle, adopting the metrical barometer and centigrade thermometer,

$$D = \frac{p}{10466{\cdot}82 \times 0{\cdot}76\,(1 + 0{\cdot}00375\,t)} \quad \ldots\ldots \quad (2)$$

It is necessary to introduce into this formula a correction for the effect of aqueous vapour. According to Watt and Saussure the weight of vapour of water is $\frac{5}{7}$ of that of dry air under the same pressure. Gay-Lussac's result, which is perhaps the more accurate, is $\frac{5}{8}$. Now if we suppose that there is in the air all the water that can exist in a state of vapour at the temperature of 86° Fahrenheit, there is then a degree of saturation greater than to be met with in nature. In this case the tension of the vapour would be 1·22 inch by the table of Dalton.

In designating this by f, p being the barometric pressure, the density of the humid atmosphere would be the same as that of dry air which would support a pressure indicated by $p - f + \frac{5}{8}f = p - \frac{3}{8}f$. Making this correction, formula (2) becomes

$$D = \frac{p - \frac{3}{8}f}{10466{\cdot}82 \times 0{\cdot}76\,(1 + 0{\cdot}00375\,t)} \quad \ldots\ldots \quad (3)$$

Introducing this value of D into the formula of Newton, we have

demicians in 1738, from experiments on sound, found it 1·4254. Laroche and Berard, 1·4954; Gay-Lussac and Welter, 1·3748; and Clement and Desormes, 1·3492: and the mean of all these is 1·4112. We have, however, used a mean of the two last only, though perhaps that of the whole is more conformable to experiments on sound.

have $\sqrt{\frac{pg}{D}} = \sqrt{10466{\cdot}82 \times 0{\cdot}76\,(1 + 0{\cdot}00375\,t)\,\frac{pg}{p - \frac{3}{8}f}}$ ..(4)

into which we must introduce the constant factor k, and

$$V = \sqrt{10466{\cdot}82 \times 0{\cdot}76\,(1 + 0{\cdot}00375\,t)\,\frac{pg}{p - \frac{3}{8}f} \times k}.$$

By extracting the square root and simplifying

$$V = \left(104{\cdot}0885 + 0{\cdot}19496\,t\right)\left(1 + \frac{3f}{16p - 6f}\right)\sqrt{g}.$$

Now if g' be the gravitating force at 45° of latitude, then at any other latitude λ, $g = g'\,(1 - 0{\cdot}00268 \cos 2\lambda)$.

From numerous observations on the length of the pendulum, we have found $g' = 9{\cdot}8058$ metres, as also the constant quantity $\frac{y}{2z} = 0{\cdot}00268$ in the usual formulæ relating to the pendulum, or the excess of the polar above the equatorial pendulum divided by twice the length of the latter. By introducing these quantities there will be obtained, after a little reduction,

$$V = \left(104{\cdot}0885 + 0{\cdot}19496\,t\right)\left(1 + \frac{f}{5\frac{1}{3}p - 2f}\right)\left(3{\cdot}1314 - 0{\cdot}00420 \cos 2\lambda\right) \quad \ldots\ldots\ldots\ldots \quad (5)$$

the velocity of sound per second in metres.

But the velocity of wind (as I have shown in the 66th volume of the Philosophical Magazine) has an effect on the velocity of sound. Let that velocity be represented by ω, and the angle which it makes with the direction of the sound by ϕ, the complete formula becomes

$$V = \left(104{\cdot}0885 + 0{\cdot}19496\,t\right)\left(1 + \frac{f}{5\frac{1}{3}p - 2f}\right)\left(3{\cdot}1314 - 0{\cdot}0042 \cos 2\lambda\right) + \omega \cos\phi \quad \ldots\ldots \quad (6)$$

the velocity per second in French metres.

When the English barometer and Fahrenheit's thermometer are used,

$V = \left\{104{\cdot}0885 + 0{\cdot}10831\,(t - 32°)\right\}\left(1 + \frac{f}{5\frac{1}{3}p - 2f}\right)\left(10{\cdot}2738 - 0{\cdot}01378 \cos 2\lambda\right) + \omega \cos\phi$ = the velocity per second in English feet . . . . (7)

The formula in its present state will be found, it is hoped, more easy in its application than that usually given, while at the same time it possesses all the precision which the most accurate data we could procure, can give.

To conduct a series of experiments to which this formula is applicable, it is necessary to possess a complete set of instruments,

struments, to determine the exact quantities which ought to be introduced. These are, a good barometer, thermometer, hygrometer, and anemometer. Daniell's hygrometer gives by means of Dalton's table the elastic force f of aqueous vapour most readily. By the anemometer the velocity of the wind must be determined, and the angle between the direction of the sound and that of the wind must be carefully noted.

I would not have endeavoured to recommend a complete apparatus so carefully, had it not frequently happened that in pretty extensive series of operations, a necessary part of it, comprehending the hygrometer and anemometer, has been neglected. Of course a set of experiments made in such a manner cannot be strictly compared with our formulæ.

In an analysis of Mr. Goldingham's experiments made at Madras, I endeavoured, in a former Number of this Journal, to point out the necessity of attending to all the circumstances likely to affect the accuracy of such experiments. Since that time, another series has been made lately by Captain Parry and Lieut. Foster, in which the same omissions occur; and of course no accurate conclusion can be drawn with respect to the effects of barometric pressure and temperature. This is the more to be regretted, as in the hands of such accurate observers, in such a climate, where the temperature was frequently very low, formulæ answering very well for the usual state of the atmosphere would have been put to a severe test.

If all the circumstances to which allusion has now been made had been attended to, Messrs. Parry and Foster would undoubtedly have detected the mistake into which they have apparently fallen. In the Appendix to Captain Parry's Journal just published, page 86, there is given a table of the velocity of sound from observation at Port Bowen. In determining these, the states of the barometer and thermometer alone are marked. At such low temperatures it frequently happens that no moisture can be detected by Daniell's hygrometer, according to the testimony of Lieut. Foster, and consequently its effects on the velocity of sound in such a climate cannot be estimated. It is to be regretted, however, that the velocity of the wind and its direction relative to that of the sound were not ascertained; for if these had been attended to, theory and observation would probably have so nearly agreed, as to have prevented these ingenious gentlemen from publishing opinions totally at variance (if I understand them) with those of men of the first eminence. The passage I have in view, is this:

"These observations appear to indicate a decided *decrease* of velocity with an *increased* density of the atmosphere, all other circumstances being alike." Now this opinion is in direct

rect opposition to that of Newton and Laplace, as well as of every other writer of any authority with whom I am acquainted. In fact, it is almost universally admitted that the velocity increases proportionally to the square root of the barometric pressure. I have formerly shown that the velocity increases, in a mean state of the atmosphere, at the rate of about 19 feet per second, for an increase of one inch in the height of the barometer; and about one foot for an increase of one degree of temperature by Fahrenheit's thermometer. Indeed, by applying the formula which I gave in a paper on Mr. Goldingham's observations, to those of Messrs. Parry and Foster, as far as they have supplied the necessary data, theory and observation will agree to within about 20 feet per second, —a pretty strong proof that theory is not contradicted by their experiments. There is reason to believe, had the other necessary data been stated, that a still nearer agreement would have been obtained. These it is greatly to be desired will be carefully attended to by future observers.

Edinburgh, Sept. 12, 1826. WM. GALBRAITH.

XXXIII. *Notices respecting New Books.*

Preparing for Publication.

WE are requested to state that a work "On External and Continuous Existence," by the author of the Essay on Cause and Effect (mentioned in the next article), is in the press, which will controvert the doctrine of Bishop Berkeley, and of Professor Fichte of Königsberg, and all the true idealists, and tend to establish a system of metaphysics more resembling that of Professor Kant.

Just Published.

"Essay on the Relation of Cause and Effect," controverting the doctrine of Mr. Hume concerning the nature of that relation; with observations on the opinions of Dr. Brown and Mr. Lawrence connected with the same subject.

On Galvanism, with Observations on its Chemical Properties and Medical Efficacy in Chronic Diseases; with Practical Illustrations. Also, Remarks on some Auxiliary Remedies, with Plates. By M. La Beaume, F.L.S., Medical-galvanist, Surgeon-electrician, &c.

Mr. Wm. Phillips will shortly publish a new and improved edition of his Outlines of Mineralogy and Geology, for the Use of Young Persons.

ANALYSIS OF PERIODICAL WORKS ON NATURAL HISTORY.

J. D. C. Sowerby's Mineral Conchology. No. LXXXVII.

A singular new genus, *Pachymya*, from the chalk with quartzose grains at Dowlands, the lowest part of the chalk formation in the vicinity of Lyme Regis, is described in this number. It is thus characterized: Shell bivalved, transversely elongated, very thick, sub-bilobate, with the beaks near the anterior * extremity. Ligaments partly immersed, attached to prominent processes or fulcra:" the species figured is *P. gigas.*—To the genus *Orbicula,* before containing two recent species only, three fossil species are added, one from the Alum clay, another probably from the Oxford clay, and a third from the Ancliffe oolite. Five *Trigoniæ* from the last-mentioned formation and the Green-sand are also figured; with two *Paludinæ* from the Weald Clay and the Hastings Sand.

XXXIV. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

JAN. 16 (*continued*). M. Ramond read a memoir on the state of vegetation at the summit of the Pic-du-Midi.—M. G. St. Hilaire read a paper entitled Zoological and physiological considerations relative to a new genus of monsters called *hypognalle,* and established for three species of double-headed calves, with heads opposed to each other and attached together by the symphysis of their lower jaws.

Jan. 23.—The same anatomist made a verbal report on Dr. Granville's memoir on an Egyptian mummy †. MM. Huzard, Chaussier and Magendie gave a favourable report on M. Girard's memoir relative to the inguinal herniæ of ruminating and monodactyl animals.—M. Audouard read a memoir or critical examination of the prevailing opinions on the origin and causes of the yellow fever.—MM. Cauchy, Ampère and Legendre, reported favourably on M. Poncelet's memoir on the centre of harmonic means.

Jan. 30.—M. Paul Laurent presented a memoir on a new process for drawing on stone.—A memoir by M. Aug. de St. Hilaire was read, On the system of agriculture adopted by the Brazilians, and its results in the province of Minas-Geraes.—A memoir by Mons. B. de Chateauneuf was read, On changes

* "We purpose in this volume," Mr. Sowerby observes, "to use the terms *anterior* and *posterior* in their correct sense, as pointed out by the situation of the mouth of the animal."

† For some account of this mummy, see Phil. Mag. vol. lxvi. p. 70.

undergone

undergone by the laws of mortality in Europe during the last half century.—M. Deleau read a memoir on the deaf and dumb who have lately recovered their hearing, and on the means of assisting those unfortunate persons.

Feb. 6.—M. Rozière, attached to the Egyptian commission, communicated a partly inedited work on the physical constitution of Egypt, and on its relations with the ancient institutions of that country.—M. Paulet communicated a paper on the yellow fever, in competition for the Montyon prize.—M. Theodore Olivier transmitted a memoir entitled Construction of mill-work, in which the axes of two toothed wheels are not situated in the same plane.—Dr. Surun of Limoges communicated a memoir on *fœtus* without heads.—M. G. de St. Hilaire read a description of the sacred crocodile, called *Suchus.* —MM. Brongniart, Brochant, Cuvier, and G. de St. Hilaire were appointed to examine a memoir by M. Marcel de Serres on the bones of the mastodon found in Languedoc.—MM. Legendre and Cauchy made a favourable report on M. Frizon's memoir relative to the summation of the similar powers of the roots of an equation, and on the theory of repeating fractions.

Feb. 13.—M. Bras, principal of the college of Libourne, submitted to the examination of the Academy his demonstration of the postulatum of Euclid, which serves as the foundation of the theory of parallels.—M. Andreossy read the first part of a memoir on the depressions of the surface of the globe in the longitudinal direction of chains of mountains, and between two adjacent coasts corresponding in their indentations. —M. Girard read a fourth memoir on canals, considered with respect to their fall and the distribution of locks on them.—M. Cauchy presented a note on the analysis of angular sections.—M. Michael Ostrogradsky transmitted a manuscript containing the demonstration of a proposition of the integral calculus which he applies to some equations, to partial differences of the second order, and to the determination of arbitrary functions.—M. Dutrochet read a memoir on the egg and the tadpole of the Batrachian reptiles. M. Leroy, of Etiolles, read some researches on asphyxia.—M. Zuglen de Nyevelt communicated a manuscript sketch of a new system of astronomy.

Feb. 20.—MM. Desfontaines and Mirbel made a very favourable report on M. Duvau's memoir on the *Veronicæ.*— M. Pinel, jun. read a memoir on the physical causes of mental alienation.—M. Segalas read a paper on the question whether the blood can be the seat of disease?

Feb. 27.—M. Girard communicated a second note on the theory

theory of heat and on corpuscular mechanism. M. Desmasieres, of Lille, communicated his microscopic and physiological researches on the genus *Mycoderma.*—M. Thenard read, for himself and M. D'Arcet, a memoir on the employment of fatty bodies as expellents of water in painting on stone and on plaster for the seasoning of low and damp places.—M. Huzard read a notice by M. Gregory on the scientific works of M. Vassali-Eandi.—M. Brousard read a memoir on the internal navigation of France.—M. Cauchy presented two memoirs, one on a new kind of calculus, resembling the infinitesimal, and the other, on the development of functions in periodical series.—M. Meirieux read a note on a lithontriptic instrument.

March 6.—M. Arago gave an account of Mrs. Somerville's experiments on the magnetism of a needle by the violet rays of the solar spectrum.—M. Laignel submitted a mechanical system for the ascending of rivers.—M. de Montlivault read a cosmological memoir on the cause and nature of the celestial motions and of light.—MM. Bosc and Duméril gave a favourable report on a memoir by MM. Quoy and Gaymard relating to the coraligenous zoophytes.

March 13.—The minister of the interior laid before the Academy a memoir, by the Agricultural Society of the Department of the Rhone, on the invention of paragrèles, and requested the Academy to make known its opinion on the probable success of these new preservatives; for which purpose it was referred to the members of the section of general physics. —M. Ramond read a memoir on the meteorology of the Pic-du-Midi.—M. Becquerel read a memoir entitled Researches on the electrical effects of contact produced by changes of temperature, and application which may be made of them for the determination of high temperatures.—MM. Fournier, Ampère and Cauchy, made a favourable report on M. Libri's memoir on the theory of numbers.—M. P. Garnier read a memoir on a new free remontoir escapement, and on a new compensation.—A memoir by M. de Beaujeu was read, entitled, Some observations on the manufacture of sugar from beet.

March 20.—M. Malmenaide presented a memoir, entitled A table of plane surfaces.—Dr. Barry read a memoir on exterior absorption.—M. Cauchy deposited a memoir on the resultant and the projections of several forces applied to the same point.

March 27.—General Brisbane transmitted to the Academy some astronomical observations made at Paramatta, in 1825. —M. Bussy was elected joint-professor at the School of

Pharmacy

Pharmacy of Paris.—M. Dupetit-Thouars read a memoir entitled An examination of two late memoirs on vegetable physiology.

April 3.—M. Pichard communicated some reflections On the molecular action of liquids on each other.—M. Warden was elected a corresponding member in the section of geography and navigation.—M. G. de St. Hilaire communicated some observations made on eggs modified in their development by exterior means.—A memoir by M. Richard was read on the pellicular tension of the surface of liquids.

April 10.—M. G. St. Hilaire continued the report of his researches in the establishment for artificial incubation at Auteuil.—MM. Latreille and Bosc gave a report on M. Dejean's memoir on the genera forming the tribe of simplicipedes of the *Carabidæ*.—A note by M. Gambart was read respecting the comet discovered on the 27th of February by M. Biala, and observed at Marseilles on the 9th of March by M. Gambart.—M. Azais read a note on the heat and the magnetism of the globe.—M. Marcel de Serres announced that he had recently found a great femur among the fossil bones at Montpellier.—M. Cauchy deposited a memoir entitled On the integration of linear equations of an equal order between two variables.

April 17.—M. Morel de Vindé made a verbal report on the treatise on the potatoe by MM. Payen and Chevalier.—M. Cauchy deposited a memoir and general formula relative to the transformation of simple integrals taken between the limits O and X of the variable.

April 24.—M. Cuvier read a report on the changes undergone by chemical theories, and on some new applications of chemistry to the useful arts.

May 1.—M. Bory de St.-Vincent read a portion of a letter addressed to him by M. Pavon, a Spanish naturalist, respecting the naturalization of cochineal in the environs of Malaga. —M. Robinet announced an apparatus invented by him for the purpose of destroying vesical calculi by means of chemical solvents.—Mr. P. N. Johnson, of London, transmitted a note relative to palladium, with three specimens of a preparation of that metal.—MM. Gay Lussac, Dulong and Ampère, gave a favourable report on M. Pouillet's memoir on the electricity of gases, and on one of the causes of the electricity of the atmosphere.

May 8.—M. Chaussier presented an anatomical specimen exhibiting a transverse fracture of the sternum at the upper third of this bone, produced during labour by the action of the sterno-pubic and sterno-mastoid muscles.—M. Cauchy deposited

posited a manuscript memoir on a new kind of integrals.—M. Fresnel, in the name of the section of physics, gave an unfavourable report on the efficacy of paragrêles.—M. Vallot presented a memoir entitled Determination of several plants mentioned by M. C. Bauhin as unknown.—M. Edwards read a memoir on the connexion between the vegetable and the animal kingdoms.—M. Brongniart, jun. read a memoir on the family of the *Bru-niaceæ.*

May 15.—M. Dumas communicated, in a letter to the Academy, a series of experiments he has made on the combinations of arsenic and on some other compounds.—MM. de Jussieu and Mirbel gave a favourable report on M. Brongniart's memoir on the *Bruniaceæ.*—M. Latreille made a verbal report on the second volume of M. Dejean's work on the species of Coleoptera in his collection.—M. Duméril made a verbal report on M. Teraube's treatise on chiromancy.

May 22.—M. Arago submitted to the Academy a fragment of an aërolite which fell in the principality of Ferrara, on the 19th of January 1824. He related the recent observations on this phænomenon of nature, and pointed out their interest in the double point of view of the chemical nature and mechanical composition of aërolites.—M. Cordier was appointed to make by means of the microscope a mechanical analysis of this stone, sent by M. Creoli, professor of physics at Bologna.

May 29.—M. Cauchy deposited a memoir on integrations. —M. Chevreul read a memoir on dyeing.

Annual public sitting on June 5: M. Poisson in the chair. At this sitting the following prizes and rewards were adjudged.

1. Prize in Experimental Physiology, founded by M. de Montyon. The Academy has decided that this cannot be awarded for the present year; but among the works submitted to its examination, it distinguishes that of Dr. Brechet, of Lyons, entitled Experimental researches on the functions of the ganglionic nervous system. This memoir contains a great number of experiments on many of the most important questions in physiology. Notwithstanding its want of arrangement and frequent lacunæ, the Academy could not hesitate to reward the author; but it has confined itself to the awarding to Dr. Brechet, by way of encouragement, the sum of 895 francs, at the same time engaging him to complete the work before he publishes it. Another work which arrested the attention of the Academy, sent from Italy by Dr. Lippi, and entitled Comparative-anatomical illustrations of the lymphatico-chyliferous system, is remarkable for the facts which it announces, and for the execution of the plates which accompany it.

it. But the committee, not having been able to verify the principal facts announced, in a satisfactory manner, have deferred the definitive judgement till the next year, reserving to Dr. Lippi the right of competition.

2. The Montyon prize for the Perfecting of the Healing Art. The examination by the Academy has been confined to the facts published between July 1821 and the end of the year 1825. By the unanimous advice of the committee, it has been determined not to award the grand prizes for 1825, but to take from the sum destined for this double object 16,000 francs, for the purpose of distribution, by way of encouragement, in the following manner. In Medical Science: 2000 francs to M. Louis, for his anatomical and pathological researches on phthisis. The Academy mentions with praise the zeal and devotion of Dr. Bailly, in his researches on the pernicious fevers of the environs of Rome; and also of MM. Audouard and Lassis, who have undertaken a series of researches to examine the causes of the yellow fever and of contagious diseases. For Surgery: the sum of 6000 fr. to M. Civiale, who has published several important memoirs on *lithontripty*, or the method of breaking vesical calculi, and who has made a great number of successful operations by its means:—2000 fr. to each of the following physicians: viz. Dr. Amussat, author of a very remarkable memoir on the structure of the urethra; Dr. Heurteloup, author of a memoir on the extraction of calculi through the urethra, who has also ingeniously perfected the necessary instruments; Dr. J. Leroy, of Etiolles, who published a work on the same subject in 1825, and first made known, in 1822 the instruments he invented and has since endeavoured to perfect; and Dr. Deleau, for having improved the catheterism of the Eustachian tube, and cured by this means several persons affected by that rare cause of deafness.

3. The Academy has not found any work to merit the Montyon prize, for the Discovery of the Means of lessening the Insalubrity of an Art or Trade.

4. The Delalande prize in Astronomy has been awarded to the work recently published by Captain Sabine, under the title of "An Account of Experiments to determine the figure of the earth by means of the pendulum vibrating seconds in different latitudes," London, 1825; which contains the results of numerous observations with the pendulum, made in the northern hemisphere, from Spitsbergen to the Portuguese island of St. Thomas.

The following prizes were proposed at this sitting of the Academy: 1. Prize in Physics for 1827; for a general and comprehensive history of the circulation of the blood in the

four classes of vertebrated animals, before and after birth, and at different ages; a gold medal of the value of 3000 francs. 2. Mathematical prize for 1827; for a method of calculating the perturbations of the elliptical motion of comets, applied to the determination of the next return of the comet of 1759, and to the motion of that observed in 1805, 1819, and 1822,—the same reward. 3. Mathematical prize proposed in 1822 and revised for 1827; first, for determining by multiplied experiments the density acquired by liquids, especially mercury, water, alcohol and sulphuric ether, by degrees of compression equal to the weight of many atmospheres; and secondly, for measuring the effects of the heat produced by such compression, the same reward. 4. Prize founded by M. Alhumbert: As no satisfactory memoirs had been received for this prize, the Academy determined that the sums destined for the prize should be united with those which will be due hereafter, to form a prize of 1200 francs, to be awarded, in 1829, to the best memoir on the following subject: A complete explanation, accompanied with figures, of the changes undergone by the skeletons and the muscles of frogs and salamanders at the different epochs of their life. 5. Montyon prize in Experimental Physiology: in 1827 the Academy will adjudge a gold medal of 895 fr. value, to the printed or manuscript work communicated to them before the 1st of January 1827, which shall appear to have contributed in the highest degree to the progress of experimental physiology. 6. Montyon prize in Mechanics: in 1827 the Academy will award a prize of 1000 fr. to the inventor or improver of instruments useful to the progress of agriculture, the mechanical arts, or the sciences. 7. Grand prizes from the Montyon legacy for the Improvement of Medicine and Surgery, and also for the Means of lessening the Unwholesomeness of any Art or Trade. The sums which will be awarded to the successful authors of discoveries or works on these subjects cannot be indicated prospectively, but they will greatly surpass the value of the highest prizes hitherto bestowed; so that the authors will be reimbursed for the expensive experiments or researches which they may undertake, and will receive recompenses proportioned to the services they may render, whether in preventing or greatly diminishing the unwholesomeness of certain professions, or in improving the medical sciences. The memoirs and instruments must be transmitted to the secretary before February 1, 1827; and the prizes will be adjudged in the public sitting of the same year. 8. The Delalande prize in Astronomy, consisting of a gold medal 625 fr. in value, will be awarded in 1827. 9. Prize in Statistics: the Academy not having found occasion

occasion to award this prize for 1825, it will be united with that for 1826, making a gold medal of 1860 fr., and adjudged in 1827.

After the adjudication and proposition of the various prizes, M. Cuvier read a historical eulogium on M. de Lacépède; M. Beudant, a memoir on the importance of the mineral kingdom with respect to its applications; M. Fourier, a historical eulogium on M. Breguet; and M. Dupin, a memoir on the sense of hearing, regarded as an instrument of measure, in application to the arts and to literature. These four works, which were listened to with interest, have since been printed.

XXXV. *Intelligence and Miscellaneous Articles.*

METEOR SEEN AT BURLINGTON. U.S.

THIS meteor was seen by Dr. Henry S. Waterhouse, about a mile south of Burlington. It disappeared at twenty minutes past eleven.

Its altitude* above the horizon, when first seen, was 9° 48′ 20″; its azimuth, as observed, was north 49° 30′ east, or, deducting 7° 36′ for the variation of the needle, its azimuth would be north 41° 54′ east. Its altitude when it went out of sight, behind a ridge of land, was 3° 6′ 20″, and its corrected azimuth north, 26° 57′ east. The place of observation is in N. latitude 44° 26′, and in longitude 73° 15′ west from Greenwich.

From its apparent magnitude to Dr. W. compared with that of the meridian sun, it must, on its first appearance, have subtended an angle of about 7 minutes, which, from a similar comparison, must have been enlarged to about twenty-eight minutes by the time of its leaving his sight. He remarked, that it seemed to him to undergo a *sudden* enlargement, at two different times, rather than a gradual one from first to last. Its tail was, at first, very small; indeed, there was scarcely any; but it increased in magnitude and splendour with great rapidity, so that when the ball went below the hill, the length of the tail was apparently equal to twenty or thirty times the diameter of the globe itself. No sparks were seen; the tail passed gradually out of sight in the direction of the main body. The light seemed equal to that of mid-day:

* From its direction across the tops of certain trees of very marked character, he was enabled to designate positions in its path, with a precision unusual in such phænomena. The necessary angles were taken by J. Johnson, Esq. of this village, and myself, with an excellent theodolite.

no report was heard. Dr. W. judged the time from its first appearance till the ball went out of his view, to be two and a half seconds, and till the tail had wholly vanished, four seconds.

These observations lead to a conclusion that it must have passed over a line very *far* to the north of this place.

The impression on the mind of Dr. W. at the time, was, that its course was nearly northwest; but of course nothing can be known on this point save with the aid of other observations, to which this notice may be auxiliary.—*Silliman's Journ.*

METEORS SEEN IN INDIA.

Colonel Blacker has given the Asiatic Society an account of a singular meteor, having the appearance of an elongated ball of fire, which he observed on the 3d of November, a little after sunset, when on the road between the Court-house and the Town-hall. Its colour was pale, for the daylight was still strong, and its larger diameter appeared greater, and its smaller less, than the semi-diameter of the moon. Its direction was from east to west, its track nearly horizontal, and altitude about thirty degrees. Colonel B. regrets not having heard of any other observation of this phænomenon at a greater distance, whereby he might have estimated its absolute height. As, however, it did not apparently move with the velocity of ordinary meteors, it was probably at a great distance, and consequently of great size. So long as Colonel Blacker beheld it, which was for five or six seconds, its motion was steady, its light equable, and its size and figure permanent. It latterly, however, left a train of sparks, soon after which it disappeared suddenly, without the attendant circumstance of any report audible in Colonel Blacker's situation.—Colonel Blacker concludes his paper with some interesting observations on luminous meteors, and considers them of perpetual occurrence, although daylight, clouds, and misty weather, so often exclude them from our view. Of their number no conception can be formed by the unassisted eye, but some conjecture may be formed of their extent from the fact mentioned by our author, that in using his astronomical telescope he has often seen what are called falling stars, shooting through the field of view, when they were not visible to the naked eye; and when it is considered that the glass only embraced one-twenty-five-thousandth part of the celestial hemisphere, it will be apparent that these phænomena must be infinitely numerous, in order to occur so frequently in so small a space.—*Calcutta Gov. Gaz.*

FLUIDITY OF SULPHUR AT COMMON TEMPERATURES.

Having placed a Florence flask containing sulphur upon a hot sand-bath, it was left to itself. Next morning, the bath being cold, it was found that the flask had broken, and in consequence of the sulphur running out, nearly the whole of it had disappeared. The flask being broken open, was examined, and was found lined with a sulphur dew, consisting of large and small globules intermixed. The greater number of these, perhaps two-thirds, were in the usual opaque solid state; the remainder were fluid, although the temperature had been, for some hours, that of the atmosphere. On touching one of these drops, it immediately became solid, crystalline, and opaque, assuming the ordinary state of sulphur, and perfectly resembling the others in appearance. This took place very rapidly, so that it was hardly possible to apply a wire or other body to the drops quick enough to derange the form before solidity had been acquired; by quick motion, however, it might be effected, and by passing the finger over them, a sort of smear could be produced. Whether touched by metal, glass, wood, or the skin, the change seemed equally rapid; but it appeared to require actual contact; no vibration of the glass on which the globules lay rendered them solid, and many of them were retained for a week in their fluid state. This state of the sulphur appears evidently to be analogous to that of water cooled in a quiescent state below its freezing point; and the same property is also exhibited by some other bodies, but I believe no instance is known where the difference between the usual point of fluidity and that which could thus be obtained is so great: it, in the present instance, amounts to 130°, and it might probably have been rendered greater if artificial cold had been applied.—M. F.—*Journal of Science.*

CRYSTALLIZATION OF SULPHUR.

The peculiar arrangement of the crystals of ice in a case of hoar frost, where every crystal appeared as if it had endeavoured to recede as far as it could from the neighbouring crystals, has been observed and described by Dr. Mac Culloch, at page 40, vol. xx. of this *Journal.* A similar effect may he pointed out as exhibited in crystallized sulphur. The man who melts and purifies the sulphur at the gunpowder works at Waltham Abbey is very expert in introducing wires or wooden forms into the melted sulphur, which, acting as nuclei cause a crystallization of sulphur as the whole cools, and then, by letting out the liquid portions, the substances introduced are found covered with acicular or prismatic crystals, at times an inch or more in length. In this way he forms letters,

letters, names, and the figures of animals, &c. In all these cases the arrangement noticed by Dr. Mac Culloch may be observed; and wherever an angle occurs, the convergence of the crystals is very striking and beautiful.—*Journal of Science.*

PRECIOUS NEPHRITE, CHINESE YU, &C.

The *kyouptsing*, called also the *modyoothwa* by the Burmese, and *yee-shulou-tse* by the Chinese, of which Dr. Abel's paper gives some account, is said to be highly prized by the Burmese, and to form a principal article of export from the Mogaon country. It is stated that large prices are given for large specimens, but that the purchasers run considerable risk, as the precious part must be sought for in the centre of the stone, and is frequently sought for in vain. The specimen which Dr. Abel examined he describes as being of a dark green, mottled or veined with a lighter green colour; of a triangular pyramidal form, of a polished surface, and as weighing 79 pounds 4 ounces troy. Whether this be the natural aspect of the mineral, or has been produced by art, Dr. Abel does not decide. From several experiments, he found its average specific gravity to be 3·03. It resisted the action of the blowpipe, excepting that it became white and brittle; when mixed with borax, and subjected to a strong heat, its colouring matter formed a hard green glass with the flux, whilst its substance formed a white enamel. The stone felt greasy, and was broken with extreme difficulty. Its fragments were very translucent on the edges. From its exterior characters, Dr. Abel was disposed to class it with nephrite, and considers it to be the oriental jade of mineralogists. A subsequent analysis of the stone, however, has satisfied him that whilst it is the mineral described under the latter name, it is, in fact, distinguished both from nephrite and prehnite, with the latter of which it has some analogy, by distinct chemical characters. He finds it composed of silica, lime, alumina, iron, manganese, and chrome, and suspects the existence of one or both of the fixed alkalis, but has not yet determined the point to his satisfaction. From nephrite he states this stone to differ in its proportion of silica, and in containing very little or no magnesia, and resembles it in the presence of chrome; from prehnite it differs in its much smaller proportion of alumina, and in the presence of chrome and manganese, but resembles it in the proportions of silica and lime. With Saussure's analysis of oriental nephrite it agrees in its general constituent character, but differs from it in the proportion of ingredients and in the presence of chrome; whether it will also be found to agree with it in the presence of potass and

and soda is yet undetermined. Another stone with which it would be interesting to compare it is the celebrated yu stone of the Chinese, which Dr. Abel, in his work on China, conjectured to be a species of nephrite closely allied to axestone, but is of opinion, from subsequent experience, that it will be found distinct from it, and probably a variety of the oriental jade. An analysis of the yu must determine this point, and no analysis that we are aware of has yet been published.

According to the second volume of the Oriental Magazine, it appears that M. Abel Rémusat, in his work entitled "Histoire de la Ville de Khotan," has determined the yu stone to be "nephrit or jade, the species called China or Oriental," and that he was confirmed in this opinion by Mr. König, of the British Museum, who has declared it to be China jade. There is reason to believe that Mons. Rémusat has fallen into a mistake on this subject, by confounding what is commonly called oriental with China jade. The former is much better known than the latter, and has been ranked with nephrite by those who would not class the China stone under the same head; thus Professor Jamieson admits an Asiatic variety of nephrite, although he refers China jade to prehnite. The minerals known in Europe under the name oriental jade are derived from India, Persia, Siberia, and even from Egypt. Mr. König might therefore state the yu to be "unquestionably the same as the substance called China jade," without thinking it the same as oriental jade. He particularly states the China jade to be allied to prehnite: but whether the two substances be the same or not, it is singular that Saussure's analysis did not satisfy Mons. Rémusat that oriental jade could not be nephrite.

SUMATRAN ORANG OUTANG.

Capt. Hull's account of a female orang of large size, taken on the south coast of Sumatra, is exceedingly interesting, in reference to the large male animal of the same species, which is described in the last volume of the Asiatic Transactions. It appears that Capt. Hull having, whilst at Bencoolen, heard of the capture of the last mentioned animal at Truman, dispatched a young man to the spot where it was taken, in the hope of his meeting with another orang of the same kind. After a lapse of several months he returned to Bencoolen, bringing with him a large female orang, as the fruit of his enterprise.

On his arrival at Truman, where he was kindly received, he heard various accounts from the natives of the animal he was in search of called by them Orang Mawah, Mawi or

Mawy.

Mawy. These animals, they said, resided in the deepest part of a forest, distant from Truman about five or six days' journey, and appeared very averse to undertake any expedition in search of them, stating that these beings would assuredly attack any small party, especially if a woman should be with them, whom they would endeavour to carry off. They were unwilling also to destroy these animals, from a superstitious belief that they are animated by the souls of their ancestors, and that they hold dominion over the great forests of Sumatra. After some days' debate, however, and hearing that a Mawah had been seen in the forest, the young man collected a party of twenty persons, armed with muskets, spears and bamboos, and having marched in an easterly direction for above thirty miles, fell in with the object of his search. The orang was sitting on the summit of one of the highest trees, with a young one in its arms. The first fire of the party struck off the great toe of the old orang, who uttered a hideous cry, and immediately lifted up her young one as high as her long arms would reach, and let it go amongst the topmost branches, which appeared too weak to sustain herself. During the time the party were cautiously approaching her to obtain another shot, the poor animal made no attempt to escape, but kept a steady watch on their movements, uttering at the time many singular sounds, and, glancing her eye occasionally towards her young one, seemed to hasten its escape by waving her hand. The second volley brought her to the ground, a ball having penetrated her breast, but the young one escaped. She measured four feet eleven inches in length, and two feet across the shoulders, and was covered with red hair. It is probable, from the spot where this animal was found being so near to Truman, that she was the mate of the one destroyed by the party from the brig. Her remains, consisting of the skin and all the bones, were transmitted home by Capt. Hull to Sir Stamford Raffles.

ON THE SUPPOSED UNICORN OF THE HIMALAYAS.

Mr. Hodgson's paper on the *chiru* concerned the animal which has been so often mentioned as the unicorn of the Himala.

The reports respecting this animal were so numerous and concurring, and so borne out by the specimens of single horns sent down at various times to the Asiatic Society, and by Bhotea drawings of a deerlike animal, with one horn springing from the centre of the forehead, that scepticism was almost silenced by the variety and quantity of evidence. The zeal of Mr. Hodgson for the advancement of knowledge, and which

which has afforded to the Asiatic Society the means of judging of the literature, antiquities, arts, and natural productions of the Himalayan region, has at length settled the question respecting the *chiru*, or antelope of the Bhoteahs. The skin and horns sent by Mr. Hodgson were the spoils of an animal which died in the menagerie of the Rajah of Nepaul, to whom it was presented by the Lama of Digurchi, whose pet it had been. The persons who brought the animal to Nepaul, informed Mr. Hodgson that the favourite abode of *chiru* is the Tingri Maidan, a fine plain or valley, through which the Arrun flows, and which is situated immediately beyond the snows by the Kooti pass; that in this valley beds of salt abound, to which the *chirus* are said to resort in vast herds. They are represented as in the highest degree wild, and inapproachable by man, flying on the least alarm; but if opposed, assuming a bold and determined front. The male and female are said to present the same general appearance.

The living subject of Mr. Hodgson's description presented none of those formidable attributes with which the tales of the Bhoteahs had clothed the *chiru*. In form and size he offered the common character of the antelope tribe, lived chiefly on grass, and did not seem dissatisfied with his captivity, although his panting showed that even the climate of Nepaul was oppressive to him; he at length sunk under a temperature which rarely exceeded 80° as a maximum, at the commencement of the hot weather. Although timid, and on his guard against the approach of strangers, he would, when warily laid hold of, submit patiently to handling.

The general form of the animal was graceful, like that of other antelopes, and was adorned with their matchless eye. His colour was reddish or fawn on the upper, and white on the lower part of the body. His distinguishing characters were, first, long sharp black horns, having a wavy triple curvature, with circular rings towards their base, which projected more before than behind: and, secondly, two tufts of hair projecting on the outer side of each nostril, together with an unusual quantity of bristles about the nose and mouth, and which gave to his head a somewhat thickened appearance. The hair of the animal resembled in texture that of all the trans-Himalayan animals which Mr. Hodgson has had the opportunity of examining, being harsh and of a hollow appearance; it was about two inches long, and so thick as to present to the hand a sense of solidity; and beneath lay a spare fleece of the softest wool.

Dr. Abel's remarks on Mr. Hodgson's paper chiefly concern the specific characters and dimensions of the animal, and

present a formal description of it drawn from the data furnished by Mr. Hodgson, and Dr. A.'s own examination of its remains. Dr. Abel proposed to call the animal, *Antelope Hodgsonii*, after its discoverer.

INDIAN RHINOCEROS.

Mr. Hodgson's observations on the rhinoceros are in continuation of a paper, read at a meeting of the Physical Committee, in February 1825, on the gestation of the rhinoceros, at the close of which he proposed to furnish to the committee, from time to time, an account of the rate of growth of one of these animals which was born in the menagerie of the Rajah of Nepaul. The first dimensions taken of the animal were made at three days old, when it measured two feet in height, three feet four inches and three-quarters in length, and four feet and seven-fourths of an inch in its greater circumference:—since that it has increased in the following proportions. From three days to one month, it gained five inches in height, five inches and three-quarters in length, and three inches and three-quarters in circumference; while from the age of one to fourteen months, it increased one foot seven inches in height, two feet in length, and two feet seven inches in circumference; from fourteen to nineteen months, four inches in height, one foot four inches and a half in length, and two feet four inches in circumference, the rhinoceros being, at the date of the last measurement, (in December 1825,) four feet four inches high, seven feet four inches and a half long, and nine feet five inches in circumference.

In general aspect the cub now resembles the mother, the heavy folds of the skin, which were wanting in July last, being fully formed in December. The nasal horn at the latter period scarcely protruded two inches beyond the skin.

The observations of Mr. Hodgson are of great value, in reference to all questions respecting the rate of development and full growth of many of the larger animals, respecting which scarcely any authentic statements are to be found in authors, although they have exercised the genius of Buffon and other philosophical writers. The diminished ratio of increase of height remarkable in the latter period of development, as stated by Mr. Hodgson, renders it probable that the animal will yet be a long time in arriving at its adult size,—a supposition which is also rendered probable by its seventeen months' gestation, and the slow growth of its horn.

Mr. Hodgson, in pursuing his inquiries, has had occasion to remark the amiableness of the young animal's disposition, both towards his keeper and strangers; an instance, he observes,

serves, of the power possessed by Asiatics, through their tranquil familiarity, of taming the most formidable quadrupeds. That the rhinoceros will submit to the domesticating influence of man we have seen more than one instance, nor would the tractability of this herbivorous animal seem in any way a matter of surprise,—when we know that the fiercest of the carnivorous tribe have become the attached companions of their master,—if the rhinoceros had not been held up by writers of every age and country as a standard of brutal and untameable fury. India exhibits numerous proofs of false conclusions by historians regarding the habits and temper of animals, and affords a field of interesting inquiry respecting their instinct, as contradistinguished to what might be called their educateable faculties. This subject has hitherto, we believe, only been treated by the naturalists of Europe, who have relied, in many cases, upon very vague or insufficient narratives, but never by any person residing in the native country of the animals whose history has been recorded.

[The preceding four articles are notices of papers read before the Asiatic Society of Bengal, from the Calcutta Government Gazette.]

NOTICES BY DR. T. FORSTER.

The Ladybirds which on the 25th, 26th, and 27th of July swarmed at Brighton, like a pestilence, were of the species *Coccinella septempunctata*, or large orange-coloured kind.

The *Phyteuma spicatum* has been found in great abundance in the parishes of Mayfield, Maresfield, and Uckfield in Sussex, this summer, so as completely to establish the habitat.

Notwithstanding the heat and dry weather of this present summer, the *Aster chinensis*, and several other oriental syngenesious plants have flowered a month later than ordinary. Nearly a quarter of an acre of ground has been covered with *Mimulus luteus*, which has flowered for several years past on a piece of boggy waste ground, near Maresfield, Sussex; but it has been discovered at length, by Dr. Forster, that a quantity of garden mould containing the haulm and probably the seeds of this plant was thrown out into the aforesaid place by a nursery-man several years ago.

Dr. Forster has observed that the number of meteors which are seen in August, are to those seen in September as 3 to 2; and that they exceed by more than three times the number seen in any other month of the year.

A correspondent will be obliged by a particular account of the great meteor seen last month at Glasgow, in Scotland, of which some imperfect descriptions have appeared in the papers.

LIST OF NEW PATENTS.

To John Ham, late of West Coker, but now of Holton-street, Bristol, vinegar-maker, for his improved process for promoting the action of the acetic acid on metallic bodies.—Dated the 13th of June 1826.—6 months allowed to enrol specification.

To Thomas John Knowlys, of Trinity College, Oxford, Esq. for an invention, communicated from abroad, of a new manufacture of ornamental metal.—13th June.—6 months.

To Thomas Halahan, of York-street, Dublin, Lieut. R.N. for his apparatus for working ordnance.—22d of June.—6 months.

To Lewis Aubrey, of Two Waters, Herts, engineer, for improvements in the web or wire for making paper.—4th of July.—4 months.

To John Poole, of Sheffield, for improvements in the steam-engine boilers or steam-generators, applicable also to the evaporation of fluids.—4th of July.—6 months.

To Daniel Freeman, of Wakefield, saddler, for improvements in measuring for, and making, collars for horses.—4th of July.—6 months.

To Peter Groves, of Liverpool-street, London, for improvements in making white-lead.—4th of July.—6 months.

To Robert Warnum, of Wigmore-street, Cavendish-square, piano-forte maker, for improvements on piano-fortes.—4th of July.—2 months.

To Peter Groves, of Liverpool-street, London, for improvements in making paint or pigment for preparing and combining a substance or material with oil, turpentine, or other ingredients.—10th of July.—6 months.

To Benjamin Lowe, of Birmingham, gilt-toy manufacturer, for improvements in useful and ornamental dressing pins.—14th of July.—2 months.

To John Guy and Jacob Harrison, of Workington, Cumberland, straw-hat manufacturers, for an improved method of preparing straw and grass to be used in the manufacture of hats.—14th of July.—6 months.

To John Palmer de la Fons, of George-street, Hanover-square, dentist, and William Littlewart, of St. Mary Axe, for their improvement in securing and mooring ships and other floating bodies, and apparatus for performing the same.—14th of July.—6 months.

To Edward Bayliffe, of Kendal, Westmoreland, worsted spinner, for improvements in the machinery for drawing, roving and spinning sheep and lambs' wool.—14th July.—6 months.

To John Lane Higgins, of No. 370, Oxford-street, for improvements

provements in the construction of cat blocks and fish-hooks, and in the application thereof.—14th of July.—6 months.

To James Barron, of Birmingham, for an apparatus for feeding fire with fuel, which apparatus is applicable to other purposes.—24th of July.—6 months.

To William Johnston, of Caroline-street, Bedford-square, for improvements on inkholders.—24th of July.—2 months.

To William Robinson, of Craven-street, Strand, for a new method of propelling vessels by steam on canals or navigable rivers, by means of a moveable apparatus attached to the stem or stern of the vessel.—24th of July.—2 months.

To William Parsons, of Portsmouth dock-yard, naval architect, for improvements in building ships or vessels calculated to lessen the dangerous effects of internal or external violence.—24th of July.—6 months.

To William Davidson, of Gallowgate, Glasgow, for a process for bleaching bees-wax, myrtle-wax, and animal tallow. —1st of August.—2 months.

To Thomas John Knowlys, of Trinity College, Oxford, and William Duesbury, of Bousal, Derby, collar manufacturer, for improvements in tanning.—1st of August.—6 months.

To Count Adolphe Eugene de Rosen, of Princes-street, Cavendish-square, for a new engine, invented abroad, for communicating power, to answer the purposes of a steam-engine. —1st of August.—6 months.

To Joseph Browne Wilks, of Tandridge Hall, Surrey, esquire, for improvements in producing steam for steam-engines, and other purposes.—2nd of August.—6 months.

To Lemuel Wellman Wright, of the Borough Road, Surrey, engineer, for improvements in the construction of trucks or carriages applicable to useful purposes.—2nd of August.— 6 months.

To John Williams, and John Doyle, both of the Commercial Road, Middlesex, for an apparatus and process for separating salt from sea-water, and thereby rendering it fresh and fit for use.—4th of August.—6 months.

Results of a Meteorological Journal for August 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.—This month has been generally calm and dry, and the weather remarkably fine, as on 21 days no measurable rain fell here. The mean temperature of the external air this month, is higher than it has been in August for the last ten years, and is three and a quarter degrees higher than the mean of that month for the same period. This high mean

mean temperature is chiefly owing to the warm and sultry nights, in which meteors were frequently seen. In the night of the 10th instant, from 9 till 12 P.M., there was a fine display of meteors in all directions, amounting to 42: the lower ones appeared the largest and most luminous, and several left long sparkling trains behind them. It is remarkable that these meteors appeared almost at regular intervals: viz. three, four, and sometimes five in quick succession about every quarter of an hour. There were dark horizontal beds of *cirrostratus* of an electrical appearance moving about at the time, which, with freshening breezes from the westward, seemed to favour their appearance, Two brilliant meteors, each about four inches in apparent diameter, were also seen here in the nights of the 18th and 27th. They descended comparatively slow from an altitude of 44 or 45 degrees, and in the mean time each separated into two distinct meteors before they disappeared. According to observations made here for some years past, meteors have been more prevalent in August, than in any other month. In August the earth has nearly arrived at its greatest warmth for the year, and the exhalations from its surface undoubtedly give additional heat to the surrounding atmosphere, in proportion to the height they ascend. Hence it should seem, by the way of reasoning on the cause of this phænomenon, that by means of this additional heat, the chemical action of the gases of which the atmosphere is composed, is excited in an unusual manner at this season of the year, and that with favourable upper currents, spontaneous accentions may easily occur, or meteors appear, from the admixture and ignition of combustible gases, aided by the prevalence of ascending and descending electrical vapours at the time. In the afternoon of the 25th it was very sultry, and soon after 5 o'clock a thunder-storm came on from the South, accompanied by a brisk gale, very vivid lightning, several loud peals of thunder, and a fine double rainbow. The flashes of lightning in the sunshine were light red, but the electrical balls and forked lightning were blue, purple, and red. Sheet lightning emanated from the clouds towards the E. and N.E. in quick succession throughout the evening and night.

Muschetoes have appeared in great numbers this month, and they are still very numerous and extremely troublesome.

The atmospheric and meteoric phænomena that have come within our observation this month, are two paraselenæ in the evening of the 17th, two solar and three lunar halos, eighty meteors, one rainbow, thunder on two different days, and sheet lightning on six evenings; and four gales of wind, namely, one from N.E., two from S.W., and one from the South.

Numerical

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·35,	Aug. 18th—Wind S.W.
	Minimum	29·60,	Ditto 25th—Wind S.

Range of the mercury . . 0·75.

	Inches.
Mean barometrical pressure for the month	29·979
——— for the lunar period ending the 4th inst. .	29·955
——— for 17 days, with the Moon in North declin.	30·006
——— for 13 days, with the Moon in South declin.	29·904
Spaces described by the rising and falling of the mercury	4·150
Greatest variation in 24 hours	0·280
Number of changes	21·

Thermometer	Maximum	83°,	Aug. 1st—Wind E.
	Minimum	51	Do. 11th—Wind W.

Range	32
Mean temp. of the external air	67·47
——— for 31 days with the Sun in Leo	67·16
Greatest variation in 24 hours	22·00
Mean temp. of spring water at 8 o'clock A.M. . . .	54·02

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air ..	98 in the evening of the 23d.
Greatest dryness of ditto . . .	40 in the aftern. of the 22d [& 27th.
Range of the index	58
Mean at 2 o'clock P.M. . . .	51·5
—— at 8 o'clock A.M. . . .	59·9
—— at 8 o'clock P.M. . . .	65·1
—— of three observations each day at 8, 2, and 8 o'clock	58·9

Evaporation for the month	4·60 inch.
Rain in the pluviameter near the ground .	1·52
Rain in ditto 23 feet high	1·39

Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 5½; fine, with various modifications of clouds, 18; an overcast sky without rain, 4½; rain, 3.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
26	17	29	1	26	18	13

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	3	2	4½	5	10	2	2½	31

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* J. CARY *in London, and Mr.* VEALL *at Boston.*

Gosport, at half-past Eight o'Clock, A.M.						Clouds.									Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
																	London.								
Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Evaporation.	Rain near the ground.	Lond. 1 P.M.	Bost. 8½ A.M.	8 A.M.	Noon.	11 P.M.	Boston 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
Aug. 1	30·06	70	53·25	54	SE.	1	1	1	...	...	1	1	...	......	30·10	29·44	70	78	68	69	...	...	Cloudy	Fine	NE.
2	29·94	72	......	60	E.	1	1	1	...	...	1	1	...	0·140	30·02	29·50	69	74	69	66	...	...	Cloudy	Cloudy	E.
3	29·89	69	......	58	N.	...	...	1	...	1	1	1	0·55	......	29·98	29·40	67	70	67	67·5	...	...	Do. th^r l^g	Cloudy, rain *p.m.*	S.
● 4	29·93	70	......	59	E.	1	...	1	...	1	1	1	...	·020	30·03	29·50	68	70	64	66	...	·03	Do. sh^{rs} n^t	Fine	NE.
5	29·96	65	......	63	NE.	1	1	1	...	1	1	...	...	......	30·05	29·55	60	67	61	65	...	...	Do.	Fine	SE.
6	30·07	64	......	62	N.	1	...	...	...	1	...	...	·45	......	30·10	29·57	64	72	64	65	...	...	Fair	Cloudy	calm
7	30·20	70	53·70	58	NW.	1	1	1	...	1	1	...	...	......	30·19	29·61	65	73	65	67	...	...	Fair	Fine	NW.
8	30·15	66	......	54	SW.	1	...	1	...	1	...	...	...	......	30·10	29·54	69	75	62	69	...	...	Fair	Fine	NW.
9	30·00	69	......	56	NE.	1	1	1	...	1	1	...	·50	......	29·96	29·44	65	74	65	66·5	...	...	Cld^y r^n n^t	Fine	SW.
10	29·95	68	......	53	NE.	1	...	1	...	1	1	1	...	......	29·95	29·52	67	71	61	65	...	...	Fair	Cloudy	NE.
☽ 11	29·90	65	......	76	SW.	1	1	1	...	1	1	1	...	·600	29·88	29·35	62	64	59	60	...	·09	Rain	Rain	W.
12	30·05	63	......	56	NW.	...	1	1	...	1	1	...	·45	......	30·05	29·55	60	67	56	60·5	...	·08	Fair	Fine, rain *a.m.*	SW.
13	30·20	66	......	59	SE.	1	...	1	...	1	1	...	...	......	30·18	29·75	61	71	62	65	...	...	Fair	Fine	W.
14	29·97	68	54·00	63	S.	1	1	1	...	1	...	...	...	......	29·95	29·46	65	71	61	64·5	...	...	Fair	Fine	W.
15	30·10	68	......	66	SW.	1	...	1	...	1	...	...	·50	......	30·10	29·60	66	73	61	62·5	...	·02	Fair	Fine, rain *a.m.*	W.
16	29·97	65	......	72	NW.	1	1	1	...	1	1	...	...	......	29·94	29·43	65	70	60	65·5	...	...	Cld^y r^n m^g	Fine	W.
17	30·12	65	......	58	W.	1	1	1	...	1	1	...	...	......	30·12	29·55	61	72	65	65	...	...	Fair	Fine Ther.	W.
○ 18	30·32	68	......	60	SW.	1	...	1	...	1	...	...	·40	......	30·30	29·70	68	76	62	70	...	...	Fine	Cloudy, 3 *p.m.* 80	W.
19	30·32	66	......	58	E.	...	...	...	1	1	...	...	...	......	30·29	29·72	69	80	69	70	...	...	Fine	Fine, 3 *p.m.* 84	SW.
20	30·08	71	54·20	60	SE.	1	1	1	...	1	...	...	...	......	30·02	29·48	74	81	65	71	...	...	Fine	Fine	calm
21	29·96	64	......	56	N.	1	1	1	...	1	...	...	·45	......	29·98	29·40	61	70	61	65	...	...	Fair	Fine	N.
22	29·95	65	......	49	SW.	1	1	1	...	...	...	...	...	......	29·94	29·55	65	71	61	62·5	...	...	Fair	Fine	calm
23	29·84	68	......	62	SW.	...	...	1	...	...	1	1	...	·385	29·73	29·24	67	71	65	68	...	...	Cloudy	Cloudy, rain *p.m.*	calm
24	29·74	67	......	56	SW.	1	1	1	...	1	1	1	·45	·015	29·76	29·18	64	72	65	63·5	...	·15	Fair r^n n^t	Fine	W.
25	29·77	71	......	57	S.	1	1	1	...	1	1	1	...	·100	29·71	29·95	68	73	65	62	...	·08	Do. th^r l^g	Rain *p.m.*	calm
☾ 26	29·78	68	54·40	54	SW.	1	...	1	...	1	...	...	...	......	29·81	29·25	65	70	60	64	...	·04	Fair	Fine	W.
27	29·98	63	......	56	SW.	1	...	1	...	1	...	...	·50	......	29·99	29·40	65	68	60	62	...	...	Fair	Fine	W.
28	29·98	68	......	60	S.	...	...	1	...	...	...	1	...	·050	30·00	29·53	65	70	65	65·5	...	...	Fair	Cloudy	SW.
29	29·90	69	......	80	S.	1	...	1	...	1	...	1	...	·025	29·90	29·38	68	72	66	68·5	...	...	Do. sho^{rs}	Cloudy	SW.
30	29·73	71	......	66	S.	1	1	1	...	1	1	1	...	·020	29·71	29·18	68	76	67	70	...	...	Do.	Fine	SW.
31	29·82	65	54·60	57	W.	1	1	1	...	1	1	1	·35	·165	29·86	29·22	62	70	64	64·5	...	...	Cld^y r^n n^t	Fine	W.
Aver.:	29·988	67·32	54·02	59·9		26	17	29	1	26	18	13	4·60	1·520	29·96	29·48	65	71	63	65·3	2·50	0·49			

THE

PHILOSOPHICAL MAGAZINE AND JOURNAL.

31st *OCTOBER* 1826.

XXXVI. *On the Methods proper to be used for deducing a general Formula for the Length of the Seconds Pendulum, from a Number of Experiments made at different Latitudes.* By J. Ivory, *Esq. M.A. F.R.S.**

THE determination of the figure of the earth by means of terrestrial experiments, is a problem of extreme delicacy. The solution of it requires the measurement of magnitudes nearly equal, such as a degree of the meridian, or a pendulum that oscillates in a given time; and as the investigation turns on the differences of those magnitudes, the greatest precision is necessary, because errors inconsiderable in respect of the magnitudes themselves, may nevertheless bear a great proportion to the small differences. But even granting that the desired degree of accuracy has been attained in the experiments, yet the difficulties of the problem are not entirely overcome. When the data of a mathematical question are exact, the result is rigorously accurate; and there can be no reason for preferring one mode of solution to another, except easiness in the calculation, or elegance in the process of deduction. But if the data be liable to error, as in most physical researches, the effect of the probable errors must be scrupulously examined in every proposed solution. Every method of investigation must be rejected as uncertain and unsafe, when small changes in the data would produce great variations in the result. Unless care be taken in this respect, it is evident that a deficiency of mathematical science might be attended with the same consequences as the want of accuracy in the experiments.

It is usual to employ the method of the least squares in deducing the ellipticity of the earth from a number of pendulum experiments made at different latitudes. Now this practice would seem at a first view to be very unexceptionable, whether we regard the certainty of the general principles of the method, or its great practical utility, or its extensive ap-

* Communicated by the Author.

plication. But no rule, however general, can be safely applied without attending in some degree to the particular circumstances of the case. And if we compare the results obtained by this method, we shall find that they often vary greatly, when no adequate reason can be assigned; which must raise our suspicions of its fitness for this particular investigation. Thus Captain Sabine, at p. 334 of his work, deduces from his 13 stations, by the method of the least squares, the equatorial pendulum x, the total increase from the equator to the pole y, and the ellipticity of the earth, viz.

$$\left.\begin{aligned} x &= 39{\cdot}01568 \\ y &= {\cdot}20213 \end{aligned}\right\}\text{inches}$$
$$\text{ellipticity} = {\cdot}00346.$$

But if we leave out the experiments at St. Thomas and Ascension, we shall obtain by calculating in the same manner from the 11 remaining stations,

$$\left.\begin{aligned} x &= 39{\cdot}01374 \\ y &= 0{\cdot}20463 \end{aligned}\right\}\text{inches}$$
$$\text{ellipticity} = {\cdot}003405.$$

And, further, if we leave out the experiments at St. Thomas, Ascension, Sierra Leone, and Jamaica, the remaining 9 stations will give us,

$$\left.\begin{aligned} x &= 39{\cdot}01257 \\ y &= 0{\cdot}206 \end{aligned}\right\}\text{inches}$$
$$\text{ellipticity} = {\cdot}00337.$$

Now in every one of the three calculations the experiments employed, being both numerous and extending from the equator to 80° of latitude, ought to be sufficient for an accurate determination. No reason, therefore, can be alleged for the great variations, except inaccuracy in the experiments themselves, or the irregular deviation of the earth's surface from the elliptical figure, or some fault in the mode of investigation by which the unavoidable errors of the experiments are greatly aggravated.

It seems necessary, from what has been said, to examine the method of the least squares in its application to the pendulum experiments. Let l, l', l'', &c. denote the pendulums found experimentally; λ, λ', λ'', &c. the latitudes of the experiments; L the equatorial, and $L+f$ the polar, pendulum: then, e, e', e'', &c. being the respective errors of the experimental quantities, we shall have,

$$\begin{aligned} L + f\sin^2\lambda - l &= e \\ L + f\sin^2\lambda' - l' &= e' \\ L + f\sin^2\lambda'' - l'' &= e'' \\ &\text{\&c.} \end{aligned}$$

As the seconds pendulum from the equator to the pole consists

sists of 39 English inches and a small variable part, it will free the arithmetical operations from the embarrassment of large numbers, if we put $l = 39 + \delta$, $l' = 39 + \delta'$, &c. $L = 39 + \Delta$: then

$$\left.\begin{array}{l} \Delta + f\sin^2\lambda - \delta = e \\ \Delta + f\sin^2\lambda' - \delta' = e' \\ \Delta + f\sin^2\lambda'' - \delta'' = e'' \\ \text{\&c.} \end{array}\right\} \quad (A)$$

In these equations the errors e, e', &c. are functions of Δ and f; and it is evident that,

$$\frac{de}{d\Delta} = 1, \quad \frac{de'}{d\Delta} = 1, \text{ \&c.}$$

$$\frac{de}{df} = \sin^2\lambda, \quad \frac{de'}{df} = \sin^2\lambda', \text{ \&c.}$$

Take the squares of both sides of every one of the equations (A), then

$$\begin{array}{l} (\Delta + f\sin^2\lambda - \delta)^2 = e^2 \\ (\Delta + f\sin^2\lambda' - \delta')^2 = e'^2 \\ \text{\&c.} \end{array}$$

Differentiate these equations, making Δ only vary, and substitute the values of the differential coefficients, $\frac{de}{d\Delta}$, $\frac{de'}{d\Delta}$, &c.: then,

$$\begin{array}{l} \Delta + f\sin^2\lambda - \delta = e \\ \Delta + f\sin^2\lambda' - \delta' = e' \\ \text{\&c.} \end{array}$$

Further, n denoting the number of the experiments, put

$$A = \frac{\delta + \delta' + \delta'' + \text{\&c.}}{n},$$

$$B = \frac{\sin^2\lambda + \sin^2\lambda' + \sin^2\lambda'' + \text{\&c.}}{n},$$

$$\sigma = \frac{e + e' + e'' + \text{\&c.}}{n},$$

then, by adding the foregoing equations, we get,

$$\Delta + Bf - A = \sigma.$$

Again, differentiate the same equations as before, making f only vary, and substitute the values of $\frac{de}{df}$, $\frac{de'}{df}$, &c.: then

$$\begin{array}{l} \Delta\sin^2\lambda + f\sin^4\lambda - \delta\sin^2\lambda = e\sin^2\lambda \\ \Delta\sin^2\lambda' + f\sin^4\lambda' - \delta'\sin^2\lambda' = e'\sin^2\lambda' \\ \text{\&c.} \end{array}$$

Put now,

$$C = \frac{\sin^4\lambda + \sin^4\lambda' + \sin^4\lambda'' + \text{\&c.}}{n},$$

$$D = \frac{\delta\sin^2\lambda + \delta'\sin^2\lambda' + \delta''\sin^2\lambda'' + \text{\&c.}}{n},$$

$$\rho = \frac{(e-\sigma)\sin^2\lambda + (e'-\sigma)\sin^2\lambda' + \text{\&c.}}{n},$$

then,

then, by adding all the equations, and observing that,

$$B\sigma + \varrho = \frac{e \sin^2 \lambda + e' \sin^2 \lambda' + e'' \sin^2 \lambda'' + \&c.}{n},$$

we shall get,

$$B . \Delta + C . f - D = B\sigma + \varrho.$$

Having now two equations containing Δ and f, we obtain from them,

$$\left.\begin{aligned} \Delta &= \frac{AC - BD}{C - B^2} + \sigma - \frac{B\varrho}{C - B^2}, \\ f &= \frac{D - AB}{C - B^2} + \frac{\varrho}{C - B^2}. \end{aligned}\right\} \quad (B)$$

It is manifest from the foregoing analysis that the suppositions $\sigma = 0$, $\varrho = 0$, correspond to the two equations,

$$e \frac{de}{d\Delta} + e' \frac{de'}{d\Delta} + e'' \frac{de''}{d\Delta} + \&c. = 0$$

$$e \frac{de}{df} + e' \frac{de'}{df} + e'' \frac{de''}{df} + \&c. = 0,$$

which determine the absolute minimum of the sum of the squares of the errors with respect to the variables Δ and f. Hence if we make $\varrho = 0$, $\sigma = 0$, in the formulæ (B), the resulting values of Δ and f will coincide with those found by the method of the least squares. In general it appears that we cannot avoid an error in Δ equal to σ, that is, to the mean of all the errors of the pendulums. But it is chiefly the terms containing ϱ that produce great changes in the relation of Δ and f, and alter notably the figure of the earth. The reason is that the divisor $C - B^2$ is always less than unit, and in many cases only a small part of unit; hence the terms in question are considerable, even when ϱ itself is very small; and as they have opposite signs in Δ and f, they have great effect in altering the ratio of those quantities, and consequently in changing the ellipticity. This argument is quite conclusive against the use of the method of the least squares for the purpose of deducing a mean result from a number of pendulum experiments.

In order to illustrate what has just been said, we may take the example before alluded to, at p. 334 of Captain Sabine's work: then

$$\begin{aligned} A &= \cdot 09107 \\ B &= \cdot 372977 \\ C &= \cdot 292646 \\ D &= \cdot 065006; \end{aligned}$$

which numbers being substituted in the formulæ (B), we shall get,

$$\begin{aligned} \Delta &= \cdot 01568 + \sigma - 2\cdot 429 \times \varrho \\ f &= \cdot 20213 + 6\cdot 513 \times \varrho. \end{aligned}$$

The suppositions $\sigma = 0$ and $\varrho = 0$ give the solution of Captain

tain Sabine, which is that of the least squares, the resulting ellipticity being ·00346. It may be observed of that gentleman's experiments, that the pendulums are too long near the equator, and in some instances the excesses are very great; on the other hand the pendulums are too short at Drontheim, Hammerfest, and Greenland. Hence it is probable that σ is negative, and ϱ positive. For the sake of illustration, let us suppose,

$$\sigma = -\cdot 001$$
$$\varrho = +\cdot 001;$$

then

$$\Delta = \cdot 01225, \; L = 39\cdot 01225$$
$$f = \cdot 20864,$$
$$\text{and the ellipticity} = \cdot 00330.$$

It is evident that a calculation, founded upon data not rigorously exact, is entirely unworthy of trust, when such minute errors produce variations of so great magnitude.

In this Journal for August last, I used a mode of calculation which, although founded on a particular arrangement of the pendulums, is not liable to the same fault as the method of the least squares. In the equations (A) suppose the least pendulum is placed first, and the rest in the order of their lengths; then subtract the first equation from every one of the rest, and there will be obtained a number of equations of condition involving f only without Δ; applying now to these equations the method of the least squares, the coefficient of f in the final equation will not be a small fraction, but, supposing the experiments combined to be well chosen, it will generally amount to several units; so that the error of the equation will not be increased but diminished in the value of f. Thus at p. 100 of the Journal cited, the coefficient of f is more than 3 in the instance of Captain Kater's experiments, and the error of the equation is diminished in the value of f in the proportion of 1 to 3. When f is known, Δ will be found by means of any one of the original equations; but it will be a great improvement of this method to deduce Δ, not from any one of the equations (A), but from the sum of them all; or, which is the same thing, to employ the equation,

$$\Delta + B \,.\, f - A = \sigma,$$

already obtained above. This method is sufficiently commodious in practice, and it will probably be found as exact as any other that can be devised.

J. Ivory.

XXXVII. *Dis-*

XXXVII. *Disquisition concerning the Length of the Seconds Pendulum, and the Ellipticity of the Earth.* By J. Ivory, Esq. M.A. F.R.S.*

In this Journal for August last, I endeavoured to show that the three principal sets of experiments with the pendulum concurred in giving to the earth very nearly the same ellipticity as that found by other methods. It must be allowed that the results I obtained agree with observation even to an unexpected degree of accuracy in two sets of the experiments; and although, in the third set, the errors are greater, yet they are warranted by the undeniable irregularities of the experiments. But the article alluded to can be considered as no more than a preliminary inquiry. The great importance of the subject requires a more definitive discussion. My present purpose is to investigate a general formula for the length of the seconds pendulum in all latitudes; to compare it with all the tolerably exact experiments that have fallen within my notice; and, by this means, to lay before the reader a precise account of the present state of this research.

In another article accompanying this, I have proved that the method of investigation I followed in this Journal for August last, is free from the objections to which the method of the least squares is liable when it is employed to deduce a mean result from a number of pendulum experiments. At p. 100 of the Journal cited, the method in question is applied to Captain Kater's experiments at the stations of the Trigonometrical Survey, joined to the experiment of Captain Sabine at Maranham, the result obtained being,

$$0 = 3{\cdot}08299 f - {\cdot}64451:$$

and if to this we add the value of Δ deduced, not from any single pendulum, but from the combination of all the pendulums, we shall get,

$$\Delta + 0{\cdot}58062 f - 0{\cdot}13314 = \sigma.$$

And hence,

$$\begin{aligned} f &= {\cdot}20905 \\ \Delta &= {\cdot}01177 + \sigma \\ L &= 39{\cdot}01177 + \sigma, \end{aligned}$$

σ denoting an unknown error of the same order with the errors of the pendulums.

In the Philosophical Transactions 1824, we find an account of experiments made by Captain Hall, R.N., and Mr. Henry Foster, with an invariable pendulum at Rio de Janeiro, and

* Communicated by the Author.

other

other places. The length of the seconds pendulum at Rio in latitude 22° 55′ 22″ S. was found as follows,

	inches.	
	39·04381,	Capt. Hall
	39·04368,	Mr. Foster
Mean	39·04374.	

Now adopting the mean result as the length of the seconds pendulum at Rio, and joining it instead of Captain Sabine's experiment at Maranham to Captain Kater's experiments, we shall find these results by the same method of calculation as before, viz.

$$0 = 1{\cdot}85347 f - 0{\cdot}38645$$
$$\Delta + 0{\cdot}59932 f - {\cdot}13709 = \sigma$$
$$f = {\cdot}20850$$
$$L = 39{\cdot}01214 + \sigma.$$

Let us now vary the experiments, as under:

Station.	Latitude.	Pendulum.
Rio de Janeiro .	22° 55′ 22″ S.	39·04374
Paramatta	33 48 43 S.	39·07696
New York . . .	40 42 43 N.	39·10168
London	51 31 8	39·13929
Unst	60 45 28	39·17146
Spitzbergen . . .	79 49 35	39·21469

Applying the same method of calculation to these six experiments, the following results will be obtained, viz.

$$0 = 1{\cdot}35189 f - 0{\cdot}28299$$
$$\Delta + 0{\cdot}53830 f - 0{\cdot}12464 = \sigma$$
$$f = {\cdot}20933$$
$$L = 39{\cdot}01196 + \sigma.$$

Finally, let us next combine a greater number of experiments, viz.

Station.	Latitude.	Pendulum.
Maranham . . .	2° 31′ 43″ S.	39·01214
Trinidad	10 38 56 N.	39·01884
Bahia	12 59 21 S.	39·02425
Madras	13 4 9 N.	39·02338
San Blas	21 32 24 N.	39·03881
Rio Janeiro . . .	22 55 22 S.	39·04374
Paramatta	33 48 43 S.	39·07696
Formentera . . .	38 39 56 N.	39·09424
New York	40 42 43	39·10168
Paris	48 50 14	39·12929
London	51 31 8	39·13929
Leith	55 58 37	39·15554
Unst	60 45 25	39·17146
Spitzbergen . . .	79 49 58	39·21469

The

The same method of calculation applied to these 14 experiments will bring out these results, viz.

$$0 = 3{\cdot}14372f - 0{\cdot}65828$$
$$\Delta + 0{\cdot}36760f - 0{\cdot}0888 = \sigma$$
$$f = {\cdot}20939$$
$$\Delta = 39{\cdot}01192 + \sigma.$$

From various combinations of the experiments made by different observers we have now obtained four results, which are extremely near one another. So very inconsiderable indeed are the differences, that one cannot help drawing the inference, That the experiments with the pendulum agree better with the figure of an elliptical spheroid than has hitherto been supposed. I have further applied the same method of calculation to 25 stations; namely, all in the following table, except the first, which is not different from London, the one place being as much south of the equator as the other is north of it: the results are these, viz.

$$0 = 8{\cdot}09462f - 1{\cdot}68657$$
$$\Delta + 0{\cdot}49585f - 0{\cdot}11508 = \sigma$$
$$f = 0{\cdot}20835,$$
$$L = 39{\cdot}01178 + \sigma.$$

From the great number of experiments combined in the calculation, the results now obtained are probably not far from the truth, and in reality we have no means of approaching near to it. Leaving out the last figure in the value of L, which nearly equalizes the sums of the positive and negative errors, we may finally adopt this formula for the length of the pendulum in all latitudes, viz.

$$l = 39{\cdot}0117 + {\cdot}20835 \sin^2 \lambda.$$

The ellipticity deduced from this formula is ·00331; and it deserves to be remarked that, in all the five calculations the ellipticity is between the quantity now set down and ·00329: it may, therefore, be estimated at ·00330, or $\frac{1}{303}$.

Having now deduced by analysis the general expression of the length of the pendulum, we must next proceed in a retrograde order, and inquire with what degree of accuracy the formula represents the phænomena. This is done in the following table, which contains 26 independent experiments, made by different observers, and extending from 51° south of the equator to 80° north of it. Every experiment is excluded from the table, the error of which is greater than $\pm{\cdot}003$ in., limits answering to $\pm 0^{mm}{\cdot}06$ with respect to the decimal pendulum in millimetres.

Falk-

Places.	Latitude.	Pendulum by experiment.	Computed Pendulum.	Excess of Calculation.	Observers.
	° ′ ″				
Falkland Islands	51 31 43 S.	39·13927	39·13942	+·00015	Duperrey*.
Paramatta	33 48 43	39·07696	39·07622	−·00074	Sir T. Brisbane†.
Rio Janeiro . ..	22 55 22	39·04374	39·04331	−·00043	Hall and Foster.
Bahia.........	12 59 21	39·02425	39·02223	−·00202	Sabine.
Maranham	2 31 43	39·01214	39·01210	−·00004	Sabine.
Trinidad........	10 38 56 N.	39·01884	39·01881	−·00003	Sabine.
Madras	13 4 9	39·02338	39·02235	−·00103	Goldingham‡.
San Blas.......	21 32 24	39·03881	39·03978	+·00097	Hall and Foster §.
Formentera.....	38 39 56	39·09424	39·09297	−·00127	Biot.
New York	40 42 43	39·10168	39·10034	−·00134	Sabine.
Figeac.........	44 36 45	39·11322	39·11447	+·00125	Biot.
Bourdeaux	44 50 26	39·11303	39·11530	+·00227	Biot.
Clermont.......	45 46 48	39·11809	39·11932	+·00123	Biot.
Paris	48 50 14	39·12929	39·12979	+·00050	Biot.
Dunnose	50 37 24	39·13614	39·13620	+·00006	Kater.
Dunkirk.........	51 2 10	39·13773	39·13767	−·00006	Biot.
London	51 31 8	39·13929	39·13938	+·00008	Kater.
Arbury Hill	52 12 55	39·14250	39·14190	−·00060	Kater.
Clifton	53 27 43	39·14600	39·14620	+·00020	Kater.
Leith	55 58 41	39·15554	39·15483	−·00071	Kater.
Portsoy	57 40 59	39·16159	39·16051	−·00108	Kater.
Stockholm	59 20 34	39·16541	39·16588	+·00047	Suanberg ‖.
Unst	60 45 28	39·17146	39·17034	−·00112	Kater.
Hammerfest	70 40 5	39·19519	39·19722	+·00203	Sabine.
Greenland......	74 32 19	39·20335	39·20525	+·00190	Sabine.
Spitzbergen	79 49 58	39·21469	39·21356	−·00113	Sabine.

The errors in this table are not great, yet in the few instances where they are considerable, it is greatly to be wished that the experiments were repeated. In particular it would contribute much to render our ideas on this subject more fixed and certain, if the French philosophers would, by new operations, determine the pendulums at Figeac, Bourdeaux and

* *Con. des Tems,* 1826. From the experiments of M. Duperrey with an invariable pendulum, the proportion of the seconds pendulum at the Falkland or Malouin Islands, to that at Paris, is 1·000255 to 1. These islands and London being in the same latitude south and north of the equator within about half a minute, the exact agreement of the two pendulums is a new proof of the delicate precision with which the experiments at the stations of the Trigonometrical Survey in Great Britain, have been executed.

† Phil. Trans. 1824; and *Con. des Tems,* 1826. ‡ Phil. Trans. 1822.

§ Phil. Trans. 1824. The length in the table is Mr. Foster's result. Capt. Hall found 39·03776 to which he gives the preference, because the operations were executed under more favourable circumstances. On the contrary, Mr. Foster's result agrees much better with other experiments, which seems to be a just ground of preference. This shows the delicate nature of these experiments; and we may learn with what caution we ought to resort to the healing power of local attraction.

‖ Swedish Transactions 1825. This pendulum was determined immediately in English measure.

 Clermont.

Clermont. If the lengths in the table turned out to be correct, we should then have one instance at least, well authenticated by actual observation, of that local attraction which is often made to play so great a part in the pendulum experiments. On the other hand if any errors should be detected in the present lengths, and the new experiments should agree better with the others that have been so successfully executed from Unst to Formentera, it would be ascertained that a great portion of the earth's surface coincides with an elliptical spheroid so nearly that it must ever elude the utmost efforts of human ingenuity to discover any difference between them. It is desirable too that all the experiments in the table were re-examined with respect to the necessary corrections, and more especially that they were all reduced to the level of the sea by one uniform rule. But the table, as it now stands, seems to prove that the earth is more regularly an elliptical spheroid than has generally been supposed.

It remains to notice the experiments excluded from the table, because the errors pass beyond the prescribed limits. They are six in number, and are with good reason placed by themselves on account of their irregularity, whether that be caused by inaccuracy of observation, or by local attraction.

Stations.	Latitude.	Observed Pendulum.	Computed Pendulum.	Excess of Calculation.	Observers.
Galapagos	0° 32′ 19″ N.	39·01717	39·01172	−·00545	Hall *.
St. Thomas	0 24 41	39·02074	39·01171	−·00903	Sabine.
Ascension..	7 55 48 S.	39·02410	39·01566	−·00844	Sabine.
Sierra Leone	8 29 58 N.	39·01997	39·01624	−·00373	Sabine.
Jamaica....	17 56 7	39·03510	39·03144	−·00366	Sabine.
Drontheim	63 25 54	39·17456	39·17838	+·00382	Sabine.

The five experiments that stand first in the table, are inconsistent with other experiments whether made near the equator, or at a distance from it, which prove that the equatorial pendulum must be about 39·01. Captain Sabine, at p. 359 of his work, assigns 39·01 as an approximation at least to the mean length of the pendulum at the equator; and, at p. 341, he seems to infer that the ellipticity will be the same, whether the equatorial pendulum be 39·01 or 39·01568. Nothing, however, is more certain than that the first of these lengths is totally inconsistent with Captain Sabine's ellipticity

* Phil. Trans. 1824. The circumstances in which this experiment was made, do not lead us to expect great precision.

and

and with all his calculations. If the equatorial pendulum be 39·01, the ellipticity computed from the pendulum at London will be ·00325; but if it be 39·01568, the ellipticity will be ·00349.

When two places are but a little different in latitude, the pendulum at one may be deduced from the pendulum at the other with considerable accuracy, even supposing that f is known approximately only. Now if we deduce the pendulum at Jamaica from those at Madras and San Blas, we shall find 39·03247 and 39·03051 instead of 39·03510 the length by experiment: and, in like manner if we compare Drontheim with Unst and Stockholm, the pendulum at the first place will be 39·17949 and 39·17789, instead of 39·17456. This indeed proves nothing directly against the accuracy of the experiments; for it may be caused by irregularity in the distribution of gravity. But it certainly ought to have some weight, since we are not forced to have recourse to vague reasoning about local attraction, in order to reconcile with one another, and with a regular figure, so many other experiments, made by different observers, and extending over a great portion of the surface of the globe. It likewise deserves to be noticed that an invariable pendulum was carried from London to Jamaica, by Mr. Campbell; and, upon the authority of experiments made with it, the seconds pendulum at Jamaica, is stated in the *Mécanique Céleste*, liv. 3^{me}, § 42, to be to that at Paris as 0·99745 to 1; from which the pendulum at Jamaica comes out 39·02952 instead of 39·03510. Making every allowance for inaccuracies in Mr. Campbell's experiments, there must remain doubts about the true length of the pendulum at Jamaica.

Sept. 30, 1826. J. IVORY.

Postscript.—In the 43d Number of the Quarterly Journal of Science, just published, there is a notice of experiments made with an invariable pendulum by M. de Freycinet in a voyage round the world. It is greatly to be regretted that the Journalist has not put us in possession of the facts of the experiments; for certainly the two columns of numbers in p. 144, give no information about the relative lengths of the pendulums at the different places. There are eight new stations, and of these, two are already in my table on good authority; three are dismissed by the writer in the Journal, as good for nothing, or not suited to his purpose; and of the remaining three, nothing can be said at present, as it is necessary to take the facts from the original work of M. de Freycinet.

Oct. 3, 1826. J. I.

XXXVIII. *Notice*

XXXVIII. *Notice of the volcanic Character of the Island of Hawaii, in a Letter to Professor* Silliman, *and of various Facts connected with late Observations of the Christian Missionaries in that Country, abstracted from a Journal of a Tour around Hawaii, the largest of the Sandwich Islands.*

[Concluded from p. 205.]

On the morning of August 1st, the party ascended from their subterranean dormitory, and directed their course N.N.E. towards the smoke. "The path, (they remark,) for several miles, lay through a most fertile tract of country, covered with bushes or tall grass, and fern from three to five feet high, and so heavily laden with dew, that before we had passed it, we were as completely wet as if we had been drawn through a river. The morning air was cool, and the singing of birds enlivened the woods. After travelling a short distance over the open country, we came to a small wood, into which we had not penetrated far, before all traces of a path entirely disappeared. We kept on some time, but were soon brought to a stand by a deep chasm, over which we saw no means of passing. Here the natives ran about in every direction, searching for marks of footsteps, just as a dog runs to and fro, when he has lost the tracks of his master. After searching about half an hour, they discovered a track, which led considerably to the southward, in order to avoid the chasm in the lava. Near the place where we crossed over, was a cave of considerable extent. In several places, drops of water, beautifully clear, constantly filtered through the arch, and fell into calabashes placed underneath to receive it. Unfortunately for us, these were all nearly empty: probably some traveller had been there but a little time previous. Leaving the wood, we entered a waste of dry sand, about four miles across. The travelling over it was extremely fatiguing, as we sunk to our ancles at every step. The sand was of a dark olive colour, fine and sparkling, adhered readily to the magnet, and being raised up in every direction, presented a surface resembling (colour excepted,) that of drifted snow. It was undoubtedly volcanic, but whether thrown out of any of the adjacent craters, in its present form, or made up of small particles of decomposed lava, and drifted by the constant trade-winds from the vast tract of lava to the eastward, we could not determine. Having refreshed ourselves, we resumed our journey, taking a northerly direction towards the columns of smoke, which we could now distinctly perceive. Our way lay over a wide waste of ancient lava, of a black colour, compact and heavy, with

with a shining vitreous surface, frequently thrown up by the expansive force of vapour or heated air, into conical mounds, from six to twelve feet high, which were rent in a number of instances from the *apex* to the *base*. The hollows between the mounds and long ridges were filled with volcanic sand, or fine particles of decomposed lava. It presented before us a sort of island sea, bounded by mountains in the distance. Once it had certainly been in a fluid state, but appeared to have become suddenly petrified, or turned into a glassy stone, while its agitated billows were rolling to and fro. Not only were the large swells and hollows distinctly marked, but in many places the surface of these billows was covered by a smaller ripple, like that observed on the surface of the sea, at the first springing up of a breeze, or the passing currents of air, which produce what the sailors call a 'cat's-paw.' The sun had risen now in his strength, and his bright rays reflected from the sparkling sand; an undulated surface of the vitreous lava dazzled our eyes, and caused considerable pain, particularly as the trade-wind blew fresh in our faces, and continually drove particles of sand into our eyes. This part of our journey was unusually laborious, not only from the heat of the sun, and the reflection from the lava, but also from the unevenness of the surface, which obliged us constantly to tread on an inclined plane, in some places as smooth, and almost as slippery, as glass, where the greatest caution was necessary to avoid a fall: frequently we chose to walk along on the ridge of a billow of lava, though considerably circuitous, rather than pass up and down its polished sides. Taking the trough or billow between the waves, we found safer, but much more fatiguing, as we sank every step deep into the sand. Between eleven and twelve o'clock we passed a number of conical hills on our right, which the natives informed us were craters. A quantity of sand was collected around their base, but whether thrown out by them, or drifted thither by the wind, they could not inform us. In their vicinity, we also passed several deep chasms, from which, in a number of places, small columns of vapour arose at different intervals. They appeared to proceed from Kirauea, the great volcano, and extended towards the sea, in a S.E. direction. Probably they are connected with Pouahohoa, and may mark the course of a vast subterraneous channel, leading from the volcano to the shore. The surface of the lava on both sides was considerably heated, and the vapour had a strong sulphureous smell.

"We continued our way, beneath the scorching rays of a vertical sun, till about noon, when we reached a solitary tree, growing in a bed of sand, and spreading its roots among the crevices

crevices of the lava. We threw ourselves down, stretched out our weary limbs beneath its grateful shade, and drank the little water left in our canteens.

"In every direction around us, we observed a number of pieces of spumous lava, of an olive colour, extremely cellular, and as light as a sponge. They appeared to have been drifted by the wind into the hollows which they occupied. The high bluff rocks on the north-west side of the volcano were very distinctly seen; the smoke and vapour driven past us, and the scent of the fumes of sulphur, which, as we approached from the leeward, we had perceived ever since the wind sprung up, were now very strong, and indicated our approach to Kirauea. Impatient to view it, we rose, after resting about half an hour, and pursued our journey. By the way-side we saw a number of low bushes, bearing beautiful red and yellow berries in clusters, each berry being about the size and shape of a large currant. The native name of the plant is *Ohelo.*

"We travelled on, clearing every Ohelo bush that grew near the path, till about 2 P.M. when the great crater of Kirauea all at once burst upon our view. We expected to have seen a mountain with a broad base, and rough indented sides, composed of loose slags or streams of lava, and whose summit would have presented a rugged wall of scoria, forming the rim of a mighty cauldron. But instead of this, we found ourselves on the edge of a steep precipice, with a vast plain before us, fifteen or sixteen miles in circumference, and sunk from 200 to 400 feet below its original level. The surface of the plain below was uneven, and strewed over with large stones, and volcanic rocks; and in the centre of it was the great crater, a mile or a mile and a half distant from the precipice, on which we were standing. Our guides led us round towards the north end of the ridge, in order to find a place by which we might descend to the plain below. As we passed along, we observed the natives, who had hitherto refused to touch any of the ohelos, now gather several bunches, and after offering a part to Pele, they ate them freely. They did not use much ceremony in their acknowledgement, but when they had plucked a bunch containing several clusters of berries, they made a stand, with their faces turned towards the place where the greatest quantities of smoke and vapour issued, and breaking the branch they held in their hand in two pieces, they threw one part down the precipice, saying at the same time, 'E Pele eia ka ohelo au; e taumaha aku wau ia oe, e ai hoi au tetaki;' ('Pele, here are your ohelos; I offer some to you, some I also eat.')

"We walked on to the north end of the ridge, where the precipice

precipice being less steep, a descent to the plain below seemed practicable. It required, however, the greatest caution, as the stones and fragments of rocks frequently gave way under our feet, and rolled down from above; and with all our care we did not reach the bottom without several falls and slight bruises. The steep which we had descended, was formed of volcanic materials, apparently a light red, and gray kind of lava, vesicular, and lying in horizontal strata, varying in thickness from one to forty feet. In a small number of places, the different strata of lava were, also, rent in perpendicular or oblique directions from the top to the bottom, either by earthquakes or other violent convulsions of the earth, connected with the action of the adjacent volcano. After walking some distance over the sunken plain, which, in several places, sounded hollow under our feet, we came suddenly to the edge of the great crater, where a spectacle, sublime and appalling, presented itself before us. Astonishment and awe for some moments deprived us of speech, and, like statues, we stood fixed to the spot, with our eyes riveted on the abyss below. Immediately before us yawned an immense gulf, in the form of a crescent, upwards of two miles in length, about a mile across, and apparently eight hundred feet deep. The bottom was filled with lava; and the south-west and northern parts of it were one vast flood of liquid fire, in a state of terrific ebullition, rolling to and fro its 'fiery surge' and flaming billows. Fifty-one craters, of varied form and size, rose, like so many conical islands, from the surface of the burning lake. Twenty-two constantly emitted columns of gray smoke, or pyramids of brilliant flame, and many of them at the same time vomited from their ignited mouths, streams of fluid lava, which rolled in blazing torrents down their black indented sides, into the boiling mass below. The sides of the gulf before us were perpendicular for about 400 feet, when there was a wide horizontal ledge of black, solid lava, of irregular breadth, but extending quite around. Beneath this black ledge, the sides sloped towards the centre, which was, as nearly as we could judge, 300 or 400 feet lower. It was evident that the crater had recently been filled with liquid lava up to this black ledge, and had, by some subterranean canal, emptied itself into the sea, or inundated the low land on the shore. The gray, and in some places apparently calcined sides of the great crater before us; the fissures which intersected the surface of the plain, on which we were standing; the long banks of sulphur, on the opposite side; the numerous columns of vapour and smoke, that rose at the north and south end of the plain, together with the ridge of steep rocks, by which it was surrounded,

surrounded, rising probably, in some places, 400 feet in perpendicular height, presented an immense volcanic panorama, the effect of which was greatly augmented by the constant roaring of the vast furnaces below.

"We then walked along the western side of the crater in search of water, which we had been informed was to be found in the neighbourhood, and succeeded in finding three pools, where the water was perfectly fresh and sweet. These pools appeared great natural curiosities. The surface of the ground in the vicinity was perceptibly warm, and rent by several deep, irregular chasms, from which steam and thick vapours continually arose. In some places these chasms were two feet wide. From thence a dense volume of steam ascended, which was immediately condensed into small drops of water, by the cool mountain air, and driven like drizzling rain into hollows in the lava, at the leeward side of the chasms. The pools, which were six or eight feet from the chasms, were surrounded and covered by flags, rushes, and tall grass. Nourished by the moisture of the vapours, these plants flourished luxuriantly, and, in their turn, sheltered the pools from the heat of the sun, and prevented evaporation. We expected to find the water warm; but in this respect we were also agreeably disappointed. When we had quenched our thirst with water thus distilled by nature, we directed the natives to build a hut for us to pass the night in, in such a situation as to command a view of the burning lava; and while they were thus employed, we prepared to examine the many interesting objects around us."

Mr. Thurston visited the eastern side of the great crater; and Messrs. Ellis and Goodrich went to examine some extensive beds of sulphur at the north-east end. After walking about three quarters of a mile over a tract of decomposed lava, covered with ohelo bushes, they came to a bank about 150 yards long, and in some places upwards of 30 feet high, formed of volcanic sulphur, with a small proportion of red clay. The ground was hot, its surface rent by fissures; and they were sometimes completely enveloped in the thick vapours that continually ascended. A number of apertures were visible along the whole extent of the bank of sulphur; smoke and vapours arose from these fissures; and the heat around them was more intense than in any other part. They climbed about half way up the bank, and endeavoured to detach some parts of the crust, but soon found it too hot to be handled. However, by means of their walking-sticks, they broke off some curious specimens. Those procured near the surface were crystallized in beautiful circular prisms of a light yellow colour, while those found three or four inches deep in the bank, were of an orange

orange yellow, generally in single or double tetrahedral pyramids, and full an inch in length.

A singular hissing and cracking noise was heard among the crystals, whenever the outside crust of sulphur was broken, and the atmospheric air admitted. The same noise was produced among the fragments broken off, until they were quite cold. The adjacent stones and pieces of clay were frequently incrusted, either with sulphate of ammonia, or volcanic sal ammoniac. A considerable quantity was also found in the crevices of some of the neighbouring rocks, which was much more pungent than that exposed to the air. Along the bottom of the sulphur bank, they found a number of pieces of tufa, extremely cellular and light. A thick fog now came over, which being followed by a shower of rain, obliged them to leave this interesting laboratory of nature, and return to their companions.

They saw flocks of wild geese, which came down from the mountains and settled among the ohelo bushes: they were informed that they were numerous in the interior, but were never seen on the coast.

At sun-setting, although the thermometer was at 69°, expecting a cold night upon the mountain, they collected fuel, and removed from a dangerous place, which the natives had superstitiously chosen for them, upon the very edge of the crater. The ground sounded hollow in every direction, frequently cracked, and in two instances actually gave way as they were passing over it, and exposed the persons, whose limbs sunk through the lava, to great danger and to some injury.

Mr. Thurston, who had been benighted at some distance, found his way back, directed by the fire, but not without experiencing great difficulty from the "unevenness of the path, and the numerous wide fissures in the lava." They now partook with cheerfulness of their evening repast, and afterwards, amidst the whistling of the winds around, and the roaring of the furnace beneath, offered up their evening sacrifice of praise. "Between nine and ten, the dark clouds and heavy fog, that, since the setting of the sun, had hung over the volcano, gradually cleared away, and the fires of Kirauea, darting their fierce light across the midnight gloom, unfolded a sight terrible and sublime beyond all they had yet seen."

"The agitated mass of liquid lava, like a flood of melted metal, raged with tumultuous whirl. The lively flame that danced over its undulating surface, tinged with sulphureous blue, or glowing with mineral red, cast a broad glare of dazzling light on the indented sides of the insulated craters,

whose

whose bellowing mouths, amidst rising flames and eddying streams of fire, shot up at frequent intervals, with loudest detonations, spherical masses of fusing lava, of bright ignited stones. The dark, bold outline of the perpendicular and jutting rocks around, formed a striking contrast with the luminous lake below, whose vivid rays, thrown on the rugged promontories, and reflected by the overhanging clouds, combined to complete the awful grandeur of the imposing scene."

They sat "gazing at the magnificent phænomenon for several hours, when they laid themselves down on mats, to observe more leisurely its varying aspect; for, although they had travelled upwards of twenty miles since the morning, and were both weary and cold, they felt little inclination to sleep. The natives, who probably viewed the scene with thoughts and feelings somewhat different from theirs, seemed however equally interested. They sat most of the night, talking of the achievements of Pele, and regarding with a superstitious fear, (at which we were not surprised,) the brilliant exhibition. They considered it the primeval abode of their volcanic deities. The conical craters, they said, were their houses, where they frequently amused themselves by playing at *konane*. The waving of the furnaces and the crackling of the flames, were the *kaui* of their *hura*, (music of their dance,) and the red flaming surge was the surf wherein they played, sportively swimming on the rolling wave."

The natives said, that according to tradition, the volcano had been burning from chaos, or night, till now—for they refer the origin of the world, and even of their gods, to chaos, or night; and the creation was, in their view, a transition from darkness to light. They stated that, in earlier ages, the volcano used to boil up, to overflow its banks, and inundate the adjacent country; but that, for many kings' reigns past, it had kept below the level of the surrounding plain, continually extending its surface, and increasing its depth, and occasionally throwing up, with violent explosion, huge rocks, or red hot stones. These eruptions, they said, were always accompanied by dreadful earthquakes, loud claps of thunder, and vivid and quick-succeeding lightning. No great explosion, they added, had taken place since the days of Keona, but many places near the sea-shore had been overflowed; on which occasions, they supposed that Pele went, by a road under ground, from her house in the crater to the shore.

The mythology of Hawaii is much interwoven with the phænomena of their volcanoes and earthquakes, and with the thunder and lightning by which they are accompanied. It is easy to trace in their absurd and extravagant fables respecting the

the contests of Pele, the goddess of volcanoes, with opposing powers, the physical conflict of fire and water, and of the various elemental agents; and certainly these fables are recommended to a poetical imagination, by much that is splendid and grand.

Whenever the natives spoke of those gods of fire, it was as "dreadful beings." They reside in all the volcanoes, but chiefly in that of Kirauea. They never travelled on journeys of mercy, but always on those of wrath. Earthquakes, thunder and lightning announced their approach: sacrifices were made to appease their anger. Hundreds of hogs, both cooked and living, were thrown into the craters, when they threatened an eruption; and during an inundation, multitudes were thrown into the rolling torrent of lava, to stay its progress.

When these infernal gods were enraged, "they filled Kirauea with lava, and spouted it out; or taking a subterraneous passage, marched to some one of their houses (craters) in the neighbourhood, and thence came down upon the delinquents, with all their dreadful scourges."

On the 2d of August, the provisions of the party being exhausted, they prepared for an immediate return; but they endeavoured previously to ascertain in the best manner they could, the size of the crater. They estimated it at 5 miles, or 5½ in circumference; but the more accurate measurement of Mr. Goodrich, mentioned in his letter, makes it 7½. The depth of the crater, they estimated at 700 or 800 feet; but Mr. Goodrich fixes it at more than 1000.

The travellers "threw down several large stones, which, after several seconds, struck on the sides, and then bounded to the bottom, where they were lost in the lava. Some of them were as large as they could lift; yet, when they reached the bottom they appeared like pebbles, and they were obliged to watch their course very steadily to perceive them at all.

The party separated into two divisions; one pursued the path along the edge of the crater, towards the sea-shore. The path was in many places dangerous, lying along narrow ridges, with fearful precipices on each side; or across deep chasms and hollows, that required the utmost care to avoid falling into them, and where a fall would have been certain death, as several of the chasms seemed narrowest at the surface. In one place they passed along for a considerable distance under a high precipice, where the impending rocks towered some hundred feet above them on their left, and the appalling flood of lava rolled almost beneath on the right. On this side they descended to small craters on the declivity, and also to the black ledge; where they collected a number of

beautiful specimens of lava, generally of a black or red colour, light, cellular, brittle, and shining. They also found a quantity of volcanic glass, drawn out into filaments as fine as human hair, and called by the natives *rouoho o Pele*, (hair of Pele). It was of a dark olive colour, semi-transparent, and brittle, though some of the filaments were several inches long. Probably it was produced by the bursting of igneous masses of lava, thrown out from the craters, or separated in fine spun thread, from the boiling fluid, when in a state of perfect fusion, borne by the smoke above the edges of the crater, and thence wafted by the winds over the adjacent plain, for they also found quantities of it at least seven miles distant from the crater. They "entered several small craters, that had been in vigorous action but a short period before, marks of very recent fusion presenting themselves on every side. Their size and height were various, and many, which, from the top, had appeared insignificant as mole-hills, they now found twelve or twenty feet high. The outsides were composed of bright shining lava, heaped up in piles of most singular form. The lava on the inside was of a light or dark red colour, with a glazed surface, and in several places, where the heat had evidently been intense, they saw a deposit of small and beautifully white crystals. They also entered several covered channels, down which the lava had flowed into the large abyss. They were formed by the cooling of the lava, on the sides and surface of the stream, while it continued to flow on underneath. As the size of the current diminished, it had left a hard crust of lava of various thicknesses over the top, supported by walls of the same materials on each side. The interior was beautiful beyond description. In many places they were ten or twelve feet high, and as many wide at the bottom. The roofs formed a regular arch, hung with red and brown stalactitic lava, in every imaginable shape; while the bottom presented one continued glassy stream. The winding of its current, and the ripple of its surface were so entire, that, it seemed as if, while in rapid motion, the stream had suddenly stopped and petrified, even before its undulated surface could subside. They travelled along one of these volcanic chambers to the edge of the precipice that bounds the great crater, and looked over the fearful steep down which the fiery cascade had rushed. In the space where it had fallen, the lava had formed a spacious basin, which, hardening as it cooled, had retained all those forms which a torrent of lava, falling several hundred feet, might be expected to produce on the viscid mass below."

Large rocks were scattered around, of four or five tons' weight,

weight, which appeared to have been thrown out in the volcanic eruptions.

Within one hundred yards of the great crater, is another of about half the size, called little Kirauea. "Its sides were covered with trees and shrubs, but the bottom was filled with lava, either fluid or scarcely cold, and probably supplied by the great crater, as the trees, &c. on its sides, showed that it had remained many years in a state of quiescence." It was stated that there were many others in the neighbourhood.

So hot are the ground and the air and vapours issuing from it, that the natives formerly cooked by these means (and it would have been considered as impious to do it by any other,) the various sacrifices offered to Pele; and even food for ordinary purposes is always cooked here, simply by burying it in the ground. This is done by the wood-cutters and by the bird-catchers.

Ascending a precipice of 400 feet in elevation, the party enjoyed an extensive view of this interesting country—of Mouna Roa and Mouna Kea, in the distance; and they could with a glass discover on Mouna Roa "numerous extinguished craters, with brown and black streams of lava, over the whole extent of its surface. The higher parts were totally destitute of vegetation, though its foot was encircled, on the side nearest to them, by trees and shrubs, which extended from its base six or seven miles."

Here they took their last view of the wide-stretched sunken plain, with all its hills and banks of sulphur, its blazing craters, and its igneous lake.

"The uneven summits of the steep rocks, that, like a wall, many miles in extent, surrounded the crater, and all its appendages, showed the original level of the country, or perhaps marked the base of some lofty mountain, originally raised by the accumulation of volcanic matter, whose bowels had been consumed by fire, and whose sides had afterwards fallen into the vast furnace, where, reduced a second time to a liquefied state, they had again been vomited out on the adjacent plain."

"But the magnificent fires of Kirauea, which they had viewed with such admiration, appeared to dwindle into taper glimmerings, when they contemplated the possible, not to say probable, existence, of immense subterranean fires immediately beneath them. The whole island of Hawaii, covering a space of 4000 square miles, from the summits of its lofty mountains, perhaps 1500 or 1600 feet above the level of the sea*, down to the beach that is washed by the rolling wave, is, according

* Admitting that snow is permanent on mountains in the torrid zone, at the

according to every observation that the travellers could make, one complete mass of lava, or other volcanic matter, in different stages of decomposition, and, perforated with innumerable apertures (or craters), forms, perhaps, a stupendous arch over one vast furnace, situated in the heart of a huge submarine mountain, of which the island of Hawaii is but the apex. Or possibly, the fires rage with augmented force, at the unfathomable depth of the ocean's bed; and reared through the superincumbent weight of waters, a hollow mountain, forming the base of Hawaii, and at the same time a pyramidal funnel, from the furnace to the atmosphere."

It seems rather remarkable that strawberries and raspberries, which usually flourish best in moist situations, should be found in Hawaii around the volcanic summits, and even in some cases in the vicinity of the crater. Within a few miles of Kirauea the travellers passed three or four high and rugged craters. One of them was said by the natives to have inundated the surrounding country about fourteen generations back. The sides of these craters are generally covered with verdure, while the broken irregular rocks on their surface "frowned like the battlements of an ancient castle in ruins." They descended from one escarpment to another, over lava more or less decomposed. One descent was 400 feet, and another 500, which brought them to "a tract of lava considerably decomposed and about five miles wide, at the end of which another steep appeared." Down this they descended "by following the course of a rugged current of lava, for about 800 feet perpendicular depth, when they arrived at the plain below, which was one extended sheet of lava, without shrub or bush, stretching to the north and south, as far as the eye could reach, and from four to six miles across, from the foot of the mountain to the sea." They crossed this flood of lava in about two hours, and arrived at a village, whose inhabitants were unwilling to believe that the travellers had not only been to Kirauea, but had broken the sulphur banks, eaten the ohelos, descended to the craters, and broken fragments of lava from them,—for Pele, they said, was a dreadful being, and would certainly have avenged the insult. They were however convinced by the sight of the specimens, but said that the travellers had escaped because they were foreigners. Pele, they said, had, only five moons ago, issued from a subterranean cavern—overflowed the low land of Kapapala—carried into

the height of 14,600 feet, it was supposed that this might be the height of Mouna Roa and Mouna Kea, as the tops of these mountains are covered with perpetual snow. Their summits are formed of decomposed lava, and contain numerous craters.

the sea some of the inhabitants, and a huge rock nearly 100 feet high, which, a little while before, had been separated by an earthquake from the main pile. They stated that it now stands in the sea, nearly a mile from shore, its bottom fixed in lava, and its summit rising considerably above the water.

The missionaries thought it probable that the eruption here alluded to, arose from "the body of the lava, which had filled Kirauea up to the black ledge—between 300 and 400 feet above the liquid lava—that it had, at the time spoken of, been drawn off by this subterranean channel, though the distance between the great crater and the land overflowed by it, was not less than thirty or thirty-five miles."

On the 3d of August, the missionaries arrived at the village of Kaimu, where they heard from the people a confirmation from eye-witnesses of the statement as to the transportation of the great rock—"they recapitulated the contest between Pele and Tamepuaa, and related the adventures of several warriors, who, with spear in hand, had opposed the volcanic demons, when coming down on a torrent of lava."

They would not believe that the travellers had dared to "break off pieces of Pele's house;" and when they saw the specimens, they were not inclined to handle them.

The missionaries observed the cracks in the ground and in the houses, produced by a recent earthquake. "Earthquakes are common over the whole island, though not so frequent in this vicinity as in the northern and western parts. They are not generally violent, except when they immediately precede the eruption of a volcano." The path from Kaimu had been smooth and pleasant; but shortly after leaving Kaimali, they passed "a very rugged tract of lava, nearly four miles across. The lava seemed as if broken to pieces while cooling; it had continued to roll on like a stream of large scoria or cinders. Their progress across it was slow and fatiguing."

As the party travelled out of Pualoa, "the lava was covered with a tolerably thick layer of soil, and the verdant plain, extending several miles towards the foot of the mountains, was agreeably diversified by groups of picturesque hills, originally craters, but now clothed with grass and ornamented with clumps of trees. The natives informed them that three of these groups, Honuaura, Malama, and Maria, being contiguous and joined at their base, arrested the progress of an immense torrent of lava, which in the days of Taiaiopu, the friend of Captain Cook, inundated all the country beyond them."

After traversing another tract of rough lava they arrived at Kapoho, situated in an amphitheatre, once evidently a crater, but now filled with people and cottages, and smiling with verdure

dure and cultivation. The centre was occupied by a brackish lake in which the children were swimming, sporting and diving.

On the 13th of August, near Waiakea, they observed three streams of fresh water that empty themselves into the bay of Waiakea—one rises among the summits of Mouna Kea, and the two others boil up through the lava, near the shore—fill several large fish-ponds, and empty into the sea.

The face of the country near Waiakea is rendered very beautiful by the frequent rains, and the long repose which this region has enjoyed from the desolating effects of volcanic eruptions.

As the travellers occasionally avoided the roughness of the land by coasting along the shores, they had opportunity to observe the bold volcanic rocks, springing up sometimes 600 feet perpendicularly from the sea—and displaying various strata of vesicular lava—from which the water was frequently seen oozing or gushing in fountains.

At Laupahoapoe they saw the ruins of a mountain of nearly 600 feet elevation, which, nine months before, had fallen into the sea in consequence of an earthquake. The cloven surface of the mountain, still in its original position, was smooth and vertical, while the fragments lay below in a state of frightful desolation, mixed with the ruins of houses, and spread for half a mile along the coast. The catastrophe, although indicated by some lambent flames that appeared at evening on the top of the rock, was so sudden, that a number of the inhabitants were involved in the consequences.

On the 25th of August, Mr. Goodrich commenced his ascent up Mouna Kea. The soil was formed of decomposed lava and ashes. At noon he dismissed his native companion, and taking his great coat and blanket, began to ascend the more steep and rugged parts. The way was difficult, on account of the volcanic rocks and stunted shrubs that covered the sides of the mountain. On his way up he found a number of red and white raspberry bushes, loaded with delicious fruit. At 5 P.M. having reached the upper boundary of the trees and bushes that surround the mountain, he erected a temporary hut, kindled a small fire, and prepared for his night's repose. The thermometer, shortly after sun-setting, stood at 43°, and the magnet, though it pointed north when held in the hand, was drawn two or three degrees to the eastward, when placed on the blocks of lava; owing, probably, to the great quantity of iron in the mountain.

After a few hours rest, he arose at eleven o'clock at night, and the moon shining brightly, he resumed his journey to-

wards

wards the summit. At midnight he saw the snow about three miles distant, directed his steps towards the place, and reached it about one o'clock on the morning of the 26th. The snow was frozen over, and the thermometer stood at 27°.

He now directed his steps towards a neighbouring peak, which appeared one of the highest; but when he had ascended it, he saw several others still higher. He proceeded towards one which appeared the highest, and bore north-east from the place where he was. On reaching the summit of this second peak, he discovered a heap of stones, probably erected by some former visitor. From this peak Mouna Roa bore south by west; Mouna Huarai, west by south; and the Island of Maui, north-west. The several hills or peaks on the summit of Mouna Kea seemed composed entirely of volcanic matter, principally cinders, pumice, and sand. Mr. Goodrich did not discover any aperture or crater on either of the summits he visited. Probably there is a large crater somewhere on the summit, from whence the scoria, sand and pumice, have been thrown out. The whole of the summit was not covered with snow. There were only frequent patches, apparently several miles in extent, over which the snow was about eight inches or a foot in thickness. The ocean to the east and west was visible, but the high land on the north and south prevented its being seen in those directions.

Mr. Goodrich commenced his descent about three o'clock; and after travelling over large beds of sand and cinders, into which he sunk more than ankle deep at every step, he reached, about sunrise, the place where he had slept the preceding evening. The descent in several places, especially over the snow, was steep and difficult, and the utmost caution was necessary to avoid a fall. In his way down, he saw at a distance several herds of wild cattle, which are very numerous in the mountains and inland parts of the island.

The natives said they were informed by their fathers, that all the land had once been overflowed by the sea, except a small peak on the top of Mouna Kea, where two human beings were preserved from the destruction which overtook the rest.

The analysis and abstract which we have now given of the journal of the missionaries, as regards the volcanic appearances in Hawaii, presents a series of facts, in the highest degree interesting and instructive. In vol. iv. at page 251, we gave a similar exhibition of the leading facts observed by Dr. J. W. Webster, and recorded in his very valuable and entertaining account of the Azores. Those observations were made and recorded by a man of science, professedly investigating

the natural history of the country where he was residing, and they certainly do much credit both to his industry and discrimination. It is with great pleasure that we add our warm commendation of the late effort of the missionaries. Situated in a remote island, in the vast expanse of the Pacific, intensely and ardently occupied in their great object,—the moral improvement and civilization of the natives;—remote from the lights of science, and subjected to physical privations both frequent and severe, we certainly owe them many thanks for the great amount of valuable information which they have, *incidentally*, contributed on the subject of the natural history of one of the most remarkable volcanic regions in the world. They have, in a very pleasing manner, blended scientific instruction with moral; and both the scientific and religious world will unite in expressing their acknowledgements to the missionaries. It is a happy illustration of the importance of uniting scientific and religious qualifications in the character of the missionary: and in our view, every important mission—especially in a *terra incognita*, (and there are many such,) should be furnished with good observers and good instruments to illustrate the different branches of natural history and of physical science. It is no offence to the higher and more appropriate objects, to add, that dignity is thus shed on the mission, both in the view of the natives and in that of the civilized communities of Christian countries. We are confident that many persons will peruse the late Journal of the missionaries, in Hawaii, because it imparts so much incidental information, while no intelligent person, of whatever feelings or sentiments, will wish the amount of that information diminished.

Mineralogy and geology, botany and zoology, astronomy and geography, philology, antiquities and history, may derive very important aid from the missionaries, as indeed valuable information has often been obtained from them in years that are past.

We are gratified also with the Journal of the tour around Hawaii, on account of the manner in which it is written. It is a manly, perspicuous, *common-sense* book, and (very judiciously in our view) omits the colloquial epithets of personal affection, with which missionaries are wont to clothe their narratives, and which, although perfectly proper in *private* communications, appear trite and formal in the view of the world.

The missionaries did not forget to avail themselves of their superior knowledge, to enlighten, as far as possible, the dark intelligence of the Hawaiians, as to the origin of volcanoes from physical causes, operating according to the laws impressed

on

on matter, by the omnipotent and all-wise Creator; and they strove by every means in their power to subvert their superstitious belief in the agency of demons of fire and earthquakes, whom it was necessary to propitiate by penances, sacrifices and privations, mingled with habitual slavish fear.

We conclude by expressing the hope that we may soon be favoured with other productions, similar to that from which we have now made such copious extracts. We trust that all who may peruse these remarks, will be inclined to read the volume of the missionaries. Besides what relates to the mission, they will find very interesting notices of the scenery of the country—of its vegetable productions, and of the manners of its inhabitants. It appears that on one occasion "the natives produced fire by rubbing two dry sticks together."

XXXIX. *On effecting Combustion by the Electric Spark. By Mr.* Thomas Howldy.

To the Editor of the Philosophical Magazine and Journal.

Sir,

ABOUT the middle of the eighteenth century it was discovered by Dr. Ludolf of Berlin, that some combustible bodies could be set on fire by means of the simple electrical spark. The first substance that he inflamed by the agency of the spark was the ethereal spirit of Frobenius (sulphuric æther?), the experiment being performed at the opening of the Royal Academy of Berlin, and "in the presence of some hundreds of persons." This interesting effect of the electric spark excited considerable attention to this part of the incipient science of electricity; and several other German philosophers, after repeating the experiment of Dr. Ludolf, pursuing the discovery, inflamed by the same agency other inflammable liquids; and even oil, pitch, and sealing-wax, when these substances had been heated nearly to the point of spontaneous combustion. The results thus obtained by the Germans were only the realization of a conjecture of M. Du Fay, a French philosopher, who had several years before conceived that the electric fluid possessed the property of inflaming combustible substances, though he was not able at the time to demonstrate the fact.

Those important experiments appear to have been effected during the year 1744; and the English philosophers having obtained the necessary information concerning them, proceeded

ceeded in the spring of the following year to repeat these experiments of the Germans, and to pursue the inquiry further. By the application of an excited glass tube to phosphorus, Dr. Miles set it on fire. And Dr. Watson fired not only inflammable air, but gunpowder, by the agency of the electric spark, when it had been previously ground with a little camphor, or a few drops of any essential oil: he also succeeded in discharging a musket, and inflaming turpentine and balsam of capivi, by the electric spark.

While philosophers were prosecuting their inquiries on this subject, the important discovery of the method of accumulating electricity upon the surface of glass was made, or at least first correctly ascertained, at Leyden in Holland. But the augmented electrical power which this discovery placed in their hands, served, at first, to create only surprise, wonder, or terror, in those who personally experienced the singularity and violence of its action. After the tumult into which the minds of philosophers were thrown by the shocks of the Leyden phial had subsided, they began to try what effects it would produce on other bodies. It is remarkable, however, that they did not resume and prosecute with much care the inquiry in which they were engaged, at the period when the brilliant discovery of the power they now possessed was made.

It is equally remarkable that the inquiry has not been prosecuted by later philosophers with much attention, if the application of the electric charge to inflammable gaseous mixtures and to the combustion or oxidation of metallic wires be excepted. But I think the *manner* in which the electric fluid has been constantly and universally applied to inflame or explode combustible bodies is the *principal cause* why the list of those bodies which have actually undergone the above processes by its agency, is so scanty. As I have practised, for a considerable time, *a new method* of producing the combustion or explosion of inflammable bodies by electricity, and have successfully extended it to several not previously subjected to the agency of the electric fluid, I intend to communicate to the public an account of it, through the medium of your Journal, provided you shall think its admission will add a little to the little already known on the subject.

The method is, in fact, founded on the same principle as the method of inflaming gunpowder, which I lately communicated to you, and is similar to it; but it requires, in most cases, a very different arrangement of the substances which are to be subjected to the agency of the electric fluid. And the experiments in general, demand a pretty exact estimate of the force of the charge, and a nice adjustment of the apparatus,

to

to render them, in every instance, successful. But every necessary direction for the successful employment of the method alluded to shall accompany the description of it when transmitted to you; which will be done as soon as I have sufficient time to look over the notes and observations made while the subject was the object of experimental inquiry.

I am, sir, your obliged servant,

Hereford, Aug. 31, 1826. THOMAS HOWLDY.

XL. *Experiments on the Strength of Cohesion of Wood. By* B. BEVAN, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

THE following table exhibits the results of some experiments I have made on the cohesive strength of various species of wood.

The specimens varied in length from 9 to 13 inches, and were reduced in a lathe for a small part of the length, near the middle, to near half an inch in diameter, leaving at each end something more in general than four inches long and about 11-10ths diameter, for the purpose of being fastened into cast-iron boxes, made of sufficient strength to bear a strain of several tons weight. The wood thus prepared was secured at each end in one of these iron boxes, and suspended vertically at the end of a lever of suitable strength to bear a force of five or six thousand pounds; the operating strain being produced by the gradual and slow motion of weights of 200 pounds each, resting occasionally at intervals, of 5, 10, 15, or 20 minutes, and sometimes for some hours. In the course of my experiments I occasionally found part of the larger ends drawn out in a cylindrical shape when the lateral adhesion was less than the longitudinal cohesion: in these cases the number of pounds expressive of the cohesion is short of what is due to the specimen, and in the table these are distinguished by +, on the other bearing. Sometimes the specimen broke during the motion of the weight, and therefore would have separated under a less force, with more time: these are marked —.

Yours, &c. &c.

B. BEVAN.

A Table

A Table of the Strength of Cohesion of Wood, from Experiments by B. Bevan, *Civil Engineer.*

Species of Wood.	Spec. Grav.	Cohesion in lbs.	Species of Wood.	Spec. Grav.	Cohesion in lbs.
1. Acacia . . .	·85	16,000+	27. Mahogany .	·80	16,500
2. Ash	·84	16,700	28. Maple . . .	·66	17,400
3. Ditto	·78	19,600	29. Mulberry .	·66	10,600
4. Beech . . .	·72	22,200	30. Oak, English	·70	19,800+
5. Birch	·64	15,000–	31. Ditto . . .	·76	15,000
6. Box	·99	15,500–	32. Ditto, old .	·76	14,000
7. Cane	·40	6,300	33. Oak pile out of the river Cam. . . .	·61	4,500
8. Cedar . . .	·54	11,400	34. Oak, black Linc. log	·67	7,700–
9. Chestnut (horse)	·61	12,100–	35. Oak, Hamboro'	·66	16,300+
10. Ditto (sweet)	·61	10,500–	36. Ditto ditto	·66	14,000
11. Damson . .	·79	14,000	37. Pine, Petersburg	·49	13,300–
12. Deal, Norway spruce	·34	18,100+	38. Do. Norway	·59	12,400–
13. Ditto ditto		17,600+	39. Ditto ditto	·66	14,300
14. Do. Christiana	·46	12,400	40. Do. Petersburg .	·55	13,100+
15. Ditto ditto	·46	12,300	41. Poplar . . .	·36	7,200–
16. Ditto ditto	·46	14,000	42. Sallow . . .	·70	18,600+
17. Do. English	·47	7,000	43. Sycamore .	·69	13,000
18. Elder . . .	·73	15,000	44. Teak, old .	·53	8,200
19. Hawthorn .	·91	10,700–	45. Walnut . .	·59	7,800
20. Ditto		9,200	46. Willow . .	·39	14,000
21. Holly	·76	16,000	47. Yew	·79	8,000
22. Laburnum .	·92	10,500			
23. Lance-wood	1·01	23,400+			
24. Lignum Vitæ	1·22	11,800			
25. Lime-tree .	·76	23,500+			
26. Mahogany .	·87	21,800+			

XLI. *A Description of* Barclay's *Hydrostatic Quadrant, and an Account of Observations made with it. By* Edward Riddle, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

Among the various improvements that have been made in nautical science and nautical instruments within the last fifty years, it has often been regretted that no instrument has hitherto been invented by which altitudes may be taken at sea, to any useful degree of exactness, independent of the horizon. The inventor of such an instrument would confer a most important benefit on all who are connected with maritime affairs, as it frequently happens even when the celestial luminaries

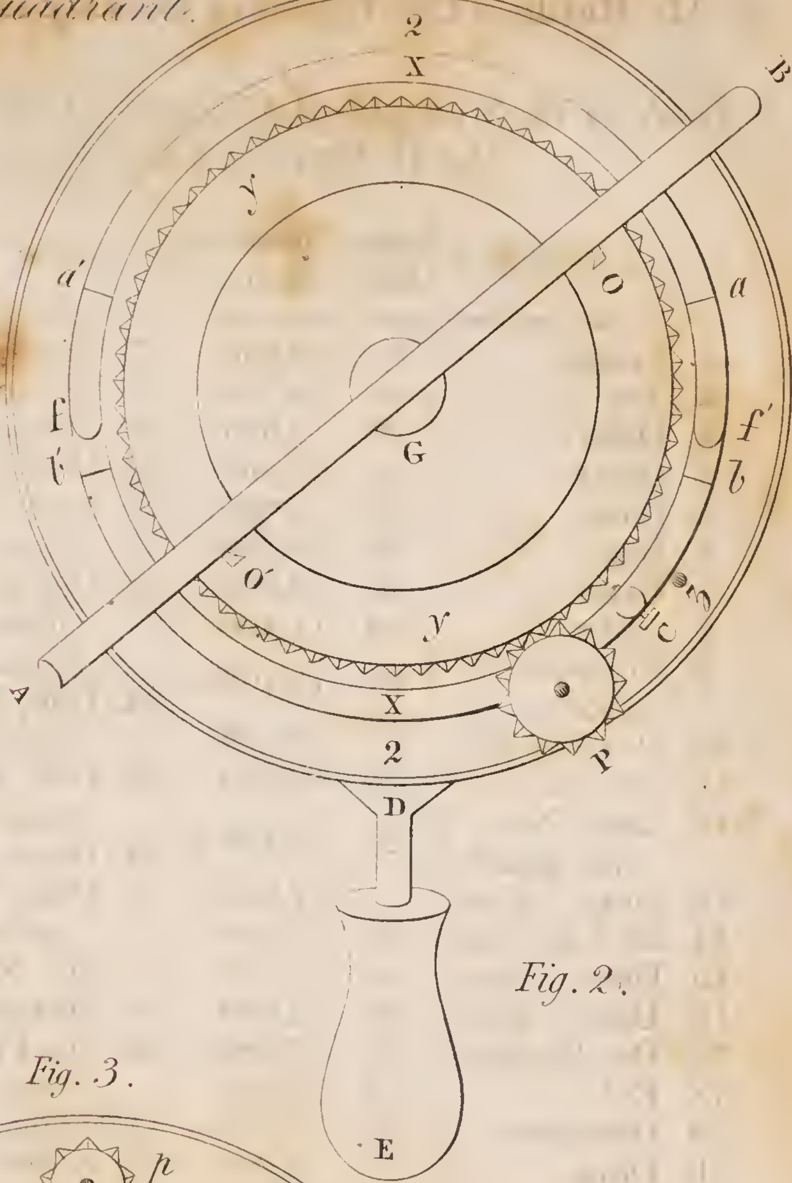


Fig. 2.

Fig. 1.

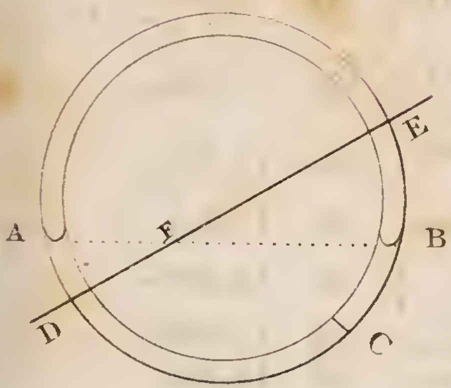
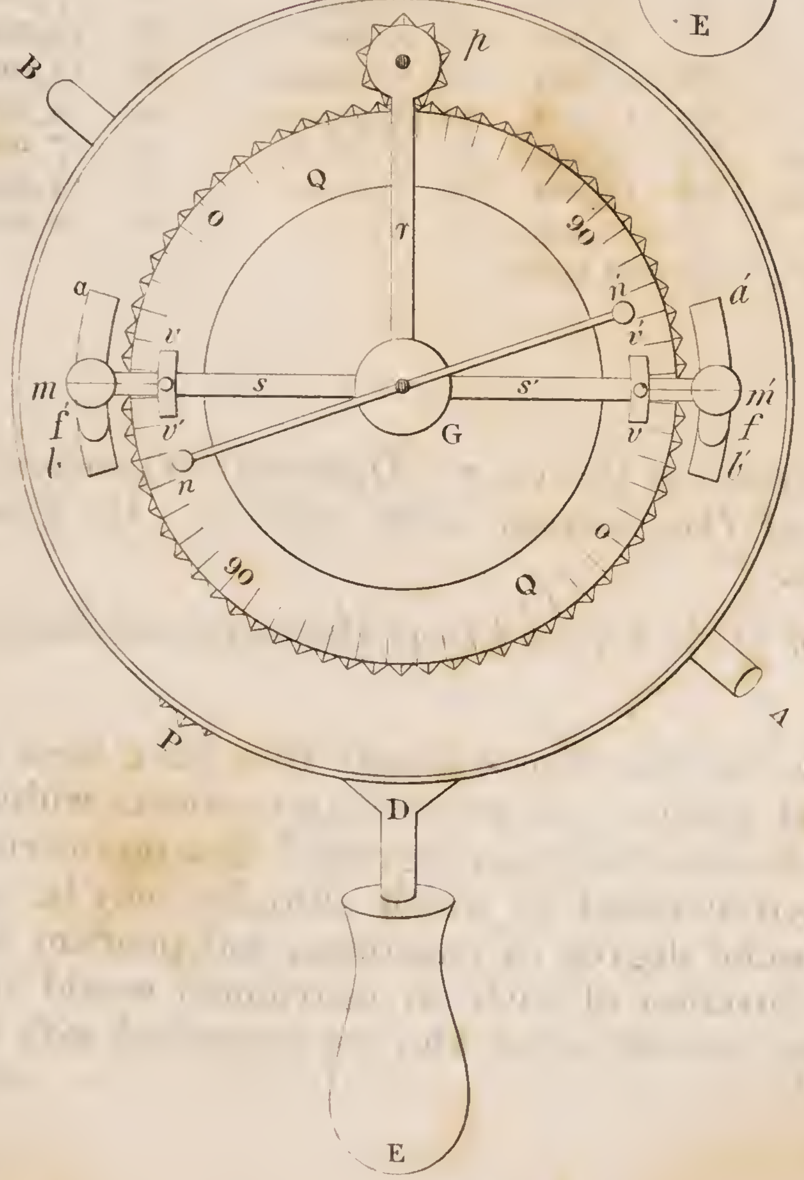


Fig. 3.



S. Porter, Sc.

luminaries are shining bright above, that from haze, fog, or darkness, the horizon cannot be seen, and the quadrant is therefore useless to the mariner, who must consequently for the time be left without any guide but his reckoning, however perilous his situation may be. How many brave hearts have quailed with anxiety in entering the Channel under such circumstances!

I have lately had put into my hands an instrument called the *Hydrostatic Quadrant*, recently invented by the Rev. Wm. Barclay, minister of Auldearn, by Nairn, in Scotland, which appears more likely to effect this desirable object than any other that has been devised for the purpose, of which I have seen any account.

Mr. Barclay, I understand, has taken out a patent for his invention; and this instrument (the only one that has yet been made) was entrusted to me, that by an extensive and varied series of observations with it on shore, I might determine what degree of accuracy might be attained by the use of it, as well as form some judgement respecting the probability of its being useful at sea.

I have accordingly made a very extensive series of observations: but before I give the results, it may be well to state the principles on which the instrument is constructed, and to give a short description of it as at present fitted up for use: and I may at once say, that should the instrument be found to realize the hopes of its inventor, I have no doubt whatever that the ingenuity of British artists will soon greatly simplify its construction.

With respect to the principle on which its operation depends,—if we conceive a circular tube, as AEBD (Plate III. fig. 1.), open all round, to be partly filled with a fluid, then it is obvious that the fluid will always occupy the lower part of the tube as ADB, and the line AFB joining the tops of the fluid will always be horizontal; and if DFE represent the axis of a telescope parallel to the plane of the tube, and pointed to any object, then, if the plane of the tube be vertical, the angle EFB, which is measured by half the sum of the arcs AD and EB, will be the altitude of the object.

But the position of the line AB will vary with every change in the position of the instrument; so that supposing the telescope pointed to an object, it becomes a question,—How are the lengths of the arcs AD and EB to be determined at that instant? This Mr. Barclay enables us to do by a very ingenious device: he inserts a *stop-cock*, as at C, in the tube, which being turned at the instant the observation is made, both the fluid and the air in the tube become fixed in their positions,

tions, and the arcs AD and EB can be read off from a graduated circle applied to the tube. So far as the mere fixing of the fluid is concerned, it is evidently of no importance in what part of the tube the stop-cock is placed: as it is placed in fig. 1. the two parts of the fluid will evidently be kept in their situations, by the intervening column of air, in any position in which, for the sake of reading, it can be required to place the instrument.—So much for the *principles* of the instrument.

The tube in the instrument that has been constructed, and which it is my purpose now to describe, is formed by grinding two equal circular rings of about three-tenths of an inch broad and seven inches interior diameter, to a sufficient depth in two circular disks of plane glass, which being firmly joined together, and made perfectly air-tight, the two cavities form the tube; and a stop-cock being inserted in it, the whole is fastened into a brass frame, the central part of both disks of glass having been previously cut out. Two brass circles firmly connected together revolve round an axis passing through the centre, one on the one side of the tube, and the other on the other. To one of these the telescope is fixed, and the circles with the telescope are moved round by a rack and pinion, the latter being attached to the frame; so that by turning the nut of the pinion the telescope can be elevated to any position. The only use of this part of the apparatus, however, is to elevate the telescope to *nearly* the required position. The circle on the other side of the tube is the graduated circle, the zeros standing at the points corresponding to the axis of the telescope, and there are two microscopes with a wire in each corresponding with the zeros of two opposite vernier scales, carried round by a pinion which works in the toothed edge of the same circle. The wires being made tangents to the top of the fluid at the point at which the tangent to the tube is a vertical line, or when the curve formed by the capillary attraction is a symmetrical one, half the sum of the readings is the required angle.

But no description of an instrument can convey so good an idea of its appearance and peculiarities, as may be obtained from a glance at a figure of it; and I doubt much whether a person who has read only the above account of this instrument will be much the wiser for it. I shall endeavour to remedy this by giving a sketch and short account of each face of the instrument; fig. 2. Plate III. representing what I shall call its *observing* face, and fig. 3. Plate III. its *reading* face.

AB (fig. 2.) is the telescope clamped to the toothed wheel *y*, *y*, at O, O′, which wheel is moved round the axis G, by the

pinion

pinion wheel P (the latter having a milled head by which it is turned). Thus the telescope can be elevated to any position. *a b*, *a' b'* are two positions of the tube X, X, which can be seen quite through, the brass on the opposite side being cut away; and *f*, *f'* represent the tops of the fluid as seen in this open space. Now the instrument being held by the handle ED as nearly vertical as the observer is able, the pinion P is turned till the telescope is pointed nearly to the required object. Then the hand which turns P, or a finger from that hand, being extended to C, the stop-cock, the whole instrument is turned in a vertical circle till the object is exactly on the horizontal wire of the telescope, and at that instant the stop-cock is brought round to the peg *z*, which is placed at the point at which the cock is close. This being done, the observation is made, and it remains only to read it, which is done on the opposite face of the instrument, represented in fig. 3.

In fig. 3. Q, Q is a circle which is firmly connected with *y*, *y* in fig. 2, and which consequently revolves with that circle and the telescope. *r*, *s*, *s'* are three radii which are all connected together and revolve on the axis G independently of Q, Q, the whole three being carried round the circle Q, Q by the pinion *p*, attached to *r*, and working in the toothed rim of Q, Q. *s* and *s'* carry two opposite microscopes *m*, *m'*, having a hair in each corresponding with the zeros of the two opposite vernier scales *v*, *v'*, which are carried on the same radii. *f*, *f'* are the two points of the fluid, seen (but on opposite sides) in fig. 2.

Now the instrument being inclined till a tangent at *f* is vertical, the capillary curve of the fluid becomes a symmetrical one, and the wire *m'* being made a tangent to the top of it in that position, the vernier shows the corresponding point on the graduated arc; and a similar operation gives the reading on the opposite side. *n*, *n'* are two microscopes for reading the verniers, which, as well as *r*, *s*, *s'*, turn round on the axis which is lengthened for the purpose.

A, B and P, in fig. 3. represent the same things as they do in fig. 2. The radii connecting *y*, *y*, Q, Q with the axis G are omitted in the figures.

It may be asked by a reader of what I have here said, Why must the micrometer wire be made a tangent to the capillary curve at the point at which it is symmetrical? as the line joining the two tops of the curve in that position can only coincide with the line joining their tops at the time of observation, when the tube is half full. I answer, That in all cases the line joining the apexes of the two symmetrical curves is *parallel* to that joining their apexes at the moment of observation, which is all that is required; and the eye can judge with

great precision when the curves on each side of the tangent at the moment of reading are equal: but it would be difficult to recollect in reading, whether the curve before you was exactly similar to one which you had seen in the opposite microscope. By inclining the instrument till the curve is symmetrical, the observer has only to make the micrometer wire a tangent to its vertex.

Enough has now perhaps been said to enable an attentive reader to understand the nature of the instrument and the method of using it; there remains however one peculiarity which I deem it of some importance to refer to. I have said in the preceding remarks, that the microscope wires m, m' in fig. 3, are opposite to each other and coincident in direction with the zeros of the verniers.

In this individual instrument, however, this is not the case. The zeros of the indexes are opposite, but the microscope wires are not so. If the wires were opposite portions of any diameter except that passing over the zeros of the verniers, they would either in reading be both behind or both in advance of the zeros, and the readings would in consequence be either both too little or both too great. It is only when they are portions of radii equally inclined to the zero diameter, that the defect in reading by the one can be compensated by a corresponding excess in the other; and they are nearly so in this instrument.

But it is an important peculiarity in the instrument, that *if the microscope wires are pointed towards the centre,* neither their relative situations with respect to the zeros of the verniers, nor the position of the axis of the telescope with respect to the zeros of the graduated circle, are matters of any importance. It was not till on finding that all the errors of my observations were *one way,* and of a certain mean value, I set about inquiring the cause, and observed that the instrument had this property, and contained within itself the means of determining the amount of any error which in observation might arise from the positions of either the microscopes or the telescopes. I had been accustomed to observe with the telescope on the right-hand side of the instrument, placing the eye as at A (fig. 2.), and I found that all my altitudes were in defect; the mean error being between three and four minutes. A little reflection, however, showed me that if the error was in the position of the telescope, or in the relative situations of the microscopes, observations made with the other face of the instrument towards the right ought to give errors a like quantity in excess; and half the difference of the errors would be, in fact, an index error applicable to observations made with either face towards the right. To

To determine this error as carefully as possible, I took the following series of altitudes of an object about a quarter of a mile distant, the instrument being kept as steadily as I possibly could in the same position.

	Telescope on right side.		Telescope on left side.
Altitudes ...	6° 50′ 30″	Altitudes ...	6° 58′ 30″
	50 30		58 0
	52 0		58 30
	50 30		61 30
	51 15		60 30
	50 0		60 30
	52 30		57 30
	52 30		57 0
	51 30		60 30
	51 0		61 30
	52 0		58 30
	52 30		58 30
Mean . . .	6 51 24		6 59 16
			6 51 24
		Diff.	7 52
		Half diff.	3 56

Hence 3′ 56″ ought to be added to all altitudes taken with the telescope on the right-hand side, and deducted from all taken with it on the left-hand side of the instrument, as it is used in observing.

Before proceeding to the observations which I have made with the instrument on celestial objects, I think it right to show that by making the microscope wires tangents to the capillary curve in that position of the instrument at which the curve is symmetrical, the readings from the instrument are sufficiently uniform among themselves to be depended on. I have made many experiments with this view, but the results of three or four will, on the present occasion, be sufficient.

I stopped the cock, and turning the telescope and circles to any position, I read in the manner above stated repeatedly, alternating the microscopes. I shall call A one of the microscropes, and B the other: then having as I have just said stopped the cock, I read as under:

A. left.	A. right.
B 39° 29′½	B 66° 15′
A 65 10	A 38 27
B 39 30	B 66 15
A 65 10	A 38 27
B 39 30½	B 66 15
A 65 10	A 38 27

I altered

I altered the position of the telescope, and read again as follows:

A. left.	A. right.
B 16° 21′	B 43° 27′
A 42 21	A 15 18
B 16 21½	B 43 27
A 42 21	A 15 18
B 16 21½	B 43 27
A 42 21	A 15 18½

Altering the telescope again, I read:

A. left.	A. right.
B 20° 39′	B 48° 19′
A 47 15	A 19 36
B 20 42	B 48 20
A 47 15	A 19 36
B 20 41	B 48 20
A 47 15	A 19 39½

These experiments will be sufficient to show that in reading from the instrument no error of any practical importance in sea affairs is to be apprehended. They show too, that it is a matter of indifference, in this instrument, with which microscope the tops of the fluid are read, provided one be read with the one and the other with the other. It will be understood that, when A in the above experiments is on the left, B is on the right, and *vice versâ*. Now it will be found that half the sum of A left and B right, differs in a degree too slight to be noticed, from half the sum of A right and B left.

The first series for example, gives

	Half sum.		Half sum.
A. left	52° 19′¾	A. right	52° 21′
	20		21
	20¼		21

The second,

A. left	29° 21′	A. right	29° 22′½
	21¼		22½
	21¼		22¾

The third,

A. left	33° 57′	A. right	33° 57′½
	58½		58
	58		59¾

The above experiments too confirm what I have said respecting the positions of the microscope wires in this instrument. If they had been exactly opposite, or portions of the same diameter, then the reading by A on the right would have

have been the same as that by B on the right; and A on the left the same as B on the left. But the readings in the first experiment give B left − A left $= 66° \; 15' - 65° \; 10' = 1° \; 5'$, and B right − A right $= 39° \; 29'\frac{1}{2} - 38° \; 27' = 1° \; 2'\frac{1}{2}$. The whole of the experiments gone through in this way give a mean of 1° 4′ 21″ for the difference between the readings of the two microscopes as applied to the same point; each of them therefore as they correct each other is part of a radius inclined to the zero diameter by about half of this quantity, or 32′ 10″.

I come now to the grand test to which I have subjected the instrument;—namely, that of comparing its results with those obtained from observations made at the same time with a sextant and artificial horizon; the latter being observed with by one of my pupils, who is remarkably dexterous in the use of the instrument. The following altitudes are of the sun's lower limb, those by the sextant being corrected for the index error of the instrument, and those by the Hydrostatic Quadrant are increased by 3′ 56″ found as above, the observations being all made with the telescope on the right.

Altitudes of ☉ observed October 5th.

Sextant.		Hydrostatic Quadrant.		Difference.
21° 40′ 41″	. . .	20° 38′ 56″	. . .	1′ 15″ −
46 36	. . .	47 26	. . .	0 50 +
52 51	. . .	55 56	. . .	3 5 +
58 51	. . .	55 56	. . .	2 55 −
22 4 6	. . .	22 3 26	. . .	0 40 −
10 11	. . .	8 56	. . .	1 15 −
15 11	. . .	15 56	. . .	0 40 +
21 11	. . .	20 26	. . .	0 45 −
25 6	. . .	25 26	. . .	0 20 +
30 41	. . .	30 26	. . .	0 15 −
31 36	. . .	33 56	. . .	2 20
40 21	. . .	41 56	. . .	1 35

October 6th.

Sextant.		Hydrost. Quad.		Difference.
14° 39′ 56″	. . .	14° 40′ 26″	. . .	0′ 30″ +
33 1	. . .	30 26	. . .	2 35 −
12 41	. . .	12 56	. . .	0 15 +
13 46 6	. . .	13 46 26	. . .	0 20 +
39 16	. . .	38 56	. . .	0 20 −
32 26	. . .	31 56	. . .	0 30 −
27 6	. . .	29 56	. . .	2 50 +
21 6	. . .	23 26	. . .	2 20 +
12 56	. . .	12 26	. . .	0 30 −

Altitudes

Altitudes of ⊙ observed October 7th.

Sextant.		Hydrostatic Quadrant.		Difference.
32° 2′ 51″	. . .	31° 59′ 56″	. . .	2′ 55″ −
1 14	. . .	32 2 26	. . .	1 12 +
31 59 54	. . .	31 59 56	. . .	0 2 +
57 49	. . .	32 0 56	. . .	3 7 +
56 24	. . .	31 55 56	. . .	0 28 −
54 14	. . .	55 56	. . .	1 42 +
52 49	. . .	55 56	. . .	3 17 +
51 49	. . .	49 56	. . .	1 53 −
50 4	. . .	47 26	. . .	2 38 −
49 49	. . .	48 56	. . .	0 53 −
28 19	. . .	28 26	. . .	0 7 +
27 4	. . .	27 26	. . .	0 22 +
25 29	. . .	22 56	. . .	2 33 −
19 49	. . .	19 56	. . .	0 7 +
17 19	. . .	15 56	. . .	1 23 −

October 8th.

Sextant.		Hydrost. Quad.		Difference.
11° 25′ 17″	. . .	11° 25′ 26″	. . .	0′ 9″ +
32 9	. . .	31 26	. . .	0 43 −
45 29	. . .	44 56	. . .	0 33 −
52 44	. . .	53 26	. . .	0 42 +
12 9 39	. . .	12 10 56	. . .	1 17 +

On the noon of October 6th I took the meridian altitude of the sun's upper limb, and found it as follows:

	A. 39° 53′			
	B. 27 31			
Sum . .	67 24			
Half sum . .	33 42			observed altitude.
		3	56″	index correction.
	33	45	56	
		1	18	refract. — parallax.
	33	44	38	
		16	2	semidiameter.
	33	28	36	true altitude.
	90			
	56	31	24	zenith distance N.
	5	0	56	declination S.
	51	30	28	latitude.

Now

Now the true latitude of this place is about 51° 28′ 52″, so that this observation was erroneous by only about a minute and a half.

It would be needless to give more examples to show that observations of the sun may be made with the instrument, on shore, to a degree of exactness generally sufficient for nautical purposes; and as in all the observations whose results are given above the instrument was held in the hand, and the observer standing without rest or support, there appears great reason to hope that it will be found capable of being used at sea. All that is required in making the observation is to be able to keep a telescope of small magnifying power pointed towards an object till the stop-cock can be shut.

I have not yet had many opportunities to take altitudes of *stars* with the instrument; but from such as I have made, I have no doubt that when there is sufficient light in the sky to show the wires in the telescope, altitudes of stars may be taken with it as easily and accurately as those of the sun. I presume that there would be little difficulty in attaching a small lamp to the instrument, by means of which, through an aperture in the side of the telescope, the wires might be made visible at any time. If this could be effected, the utility of the instrument would be greatly increased.

In the twilight of October 6th, by a mean of three observations at $6^h\ 16^m\ 58^s$ by a chronometer, I found the apparent altitude of α *Aquilæ* to be 46° 34′ 56″ by the Hydrostatic Quadrant; and at $6^h\ 20^m\ 10^s$ by a mean of two observations with the same instrument, the apparent altitude of *Arcturus* was 23° 0′ 41″; and further by a mean of two observations taken with the same instrument, I found that at $6^h\ 37^m\ 45^s$ the apparent altitude of *Arcturus* was 20° 18′ 11″.

Now on the same evening, by a mean of four observations which I took with a sextant and an artificial horizon, I found the apparent altitude of α *Andromedæ* to be 45° 34′ 22″ at $7^h\ 36^m\ 17^s{\cdot}5$; and by computing from this observation, I find that the chronometer was $15^s{\cdot}8$ fast for mean time. The error of the same chronometer as computed from the first of the preceding altitudes of *Arcturus* is 9^s, and by the second it is 14^s fast. Results agreeing so nearly with that obtained from the sextant bear strong testimony to the utility of the instrument by which they were made. Taking the mean of 9^s and 14^s, or about 11^s, as the error of the chronometer, and applying it to the time at which the altitude of α *Aquilæ* was observed, and computing the *latitude* from that star's meridian distance, polar distance and altitude, I find the latitude to be 51° 31′ 22″, being in excess by only 2′ 26″.

I have

I have now on the subject of this instrument done my duty, which has been simply to describe it, and to exhibit the results that I have obtained from it; and if it be found in practice at sea to give results at all approaching in accuracy to those which I have got, its inventor has entitled himself to the gratitude of all nautical men.

The present instrument, however, is much too heavy; but that circumstance is no objection to the principle of the instrument, as there would be no difficulty in making one of half the weight. It is difficult too in the way in which this tube is made, to prevent the fluid from leaking; but that and other mechanical difficulties in the construction of the instrument will, I doubt not, be speedily surmounted, if on trial at sea the hopes of the inventor respecting its usefulness should be realized; and it will afford me much satisfaction, if what I have here done may induce some zealous and skilful navigator to give the instrument a trial.

It has never been contemplated by the inventor to put his instrument in *competition with the quadrant.* His sole hope has been that it may be found to be a useful auxiliary, in circumstances under which that admirable instrument cannot be used.

I have only to add in conclusion, that I shall be happy to comply with the wish of the respectable and ingenious inventor, in showing the instrument to any gentleman who may favour me with a call.

I am, &c.

Greenwich Hospital, Oct. 13, 1826. EDWARD RIDDLE.

XLII. *Observations on the practical Inutility of Messrs.* YARROW *and* LYNN'S *new Methods of finding the Longitude. By* E. RIDDLE, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

A WORK has just been published entitled " New Methods of finding the Longitude either at Sea or on Shore," by Messrs. Yarrow and Lynn; the former a seafaring gentleman of considerable experience, the latter is advantageously known to the public as the author of an extensive collection of Tables calculated to facilitate nautical computations.

In Mr. Yarrow's method, from the latitude of the place, the altitudes and polar distances of the moon and any other object,

ject, he finds in the usual way their horary angles, the sum or the difference of which is the *difference of their right ascensions*, and the time deduced from the Nautical Almanac, or from the tables given in this work, when the moon and the other object have the computed difference of right ascension, is the *Greenwich time of the observation.* This is the *peculiarity* in the method; the time at the place of observation is found in the usual way.

It will readily be perceived that the method is right in theory, and if the data could be obtained at sea with sufficient exactness, it would form a very useful addition to the methods now in practice for finding the longitude. But to give results of any practical value in the present state of nautical science, both the latitude of the ship and the altitudes of the objects must be known to a degree of precision which I fear is quite unattainable in practice at sea.

It will readily be admitted by any gentleman acquainted with nautical matters, that a seaman can seldom be assured of his latitude to nearer than a minute; and from the fluctuations in the horizontal refraction, and many other circumstances connected with the taking of altitudes at sea, it may be doubted whether an altitude taken under the most favourable circumstances, from the sea horizon, ought to be relied on to less than the same quantity; and with respect to stars the limits of uncertainty will generally be much wider.

For the purpose of showing in a simple manner, what degree of uncertainty may exist in the longitude as determined by this method at sea, I have computed the two following tables: the first of which shows the error in the horary angle resulting from an error of one mile in the ship's latitude; and the second, the error in the horary angle arising from an error of one minute in the altitude. Both tables are computed for every 10° of latitude and azimuth as far as 80°.

Let H = the horary angle, H' its increment; L = the latitude, L' its increment, a = the altitude, a' its increment; and A = the azimuth.

Then we have the following well known equations:

$$H' = L' . \text{ sect } L . \cot A.$$
$$H' = a' . \text{ sect } L . \text{ cosect } A.$$

The first of the following tables was computed from the former of these formulæ, and the second from the latter.

TABLE I.

Azim.	10°	20°	30°	40°	50°	60°	70°	80°
Lat.	Error in Hour Angle for 1′ error in Latitude.							
10°	5′ 46″	2′ 47″	1′ 46″	1′ 13″	0′ 51″	0′ 35″	0′ 22″	0′ 11″
20	6 2	2 55	1 50	1 15	0 54	0 37	0 23	0 11
30	6 33	3 10	2 0	1 22	0 58	0 40	0 25	0 12
40	7 24	3 35	2 16	1 33	1 6	0 45	0 28	0 14
50	8 49	4 16	2 41	1 51	1 18	0 54	0 34	0 16
60	11 20	5 30	3 28	2 23	1 41	1 9	0 44	0 21
70	16 35	8 2	5 4	3 29	2 27	1 41	1 4	0 31
80	32 40	15 49	9 59	6 52	4 50	3 22	2 6	1 1

TABLE II.

Azim.	10°	20°	30°	40°	50°	60°	70°	80°
Lat.	Error in Hour Angle for 1′ error in Altitude.							
10°	5′ 51″	2′ 58″	2′ 2″	1′ 35″	1′ 19″	1′ 10″	1′ 5″	1′ 2″
20	6 7	3 7	2 8	1 39	1 23	1 14	1 8	1 5
30	6 39	3 22	2 18	1 48	1 30	1 20	1 14	1 10
40	7 31	3 49	2 37	2 2	1 42	1 30	1 23	1 19
50	8 58	4 33	3 7	2 26	2 2	1 48	1 39	1 34
60	11 31	5 51	4 0	3 7	2 37	2 19	2 8	2 2
70	16 50	8 33	5 51	4 33	3 49	3 23	3 6	2 58
80	33 10	16 50	11 31	8 58	7 31	6 39	6 7	5 51

Now we shall suppose as an example, that the sun and the moon are the objects observed, the latitude of the ship being one minute uncertain, and the altitude of each object also one minute uncertain; that the latitude of the ship is about 50°, the azimuth of the sun 40°, and of the moon 50°. Then in Table I. we have the uncertainty in the solar horary angle arising from the uncertainty in the latitude, 1′ 51″, and the uncertainty in the lunar horary angle arising from the same cause, 1′ 18″. And in Table II. we have the uncertainty in the same angles resulting from the uncertainty in the altitudes, 2′ 26″ and 2′ 2″ respectively. Hence the sum of these four arcs, or 7′ 37″, is the uncertainty in the difference of the right ascensions arising from an uncertainty of only one mile in the ship's latitude, and one minute in the altitudes of the objects. This in a general way would render the longitude uncertain to about $3°\frac{3}{4}$.

In

In the lunar method the uncertainty arising from this cause would have been simply the sum of two of these numbers, one from each table; namely, 4′ 17″, or 3′ 20″, according as the ship time might be found from the altitude of the sun or the moon.

If in this example the latitude and altitudes could be supposed uncertain only a *quarter of a minute*, even then would the longitude be uncertain by nearly a whole degree; an uncertainty so much greater than could possibly exist in the result as determined by a Lunar Observation, if taken by a person of even the most moderate skill in observing,—that simple as the method is in theory, and easy as it would appear in practice, it cannot, as I think, be adopted at sea with any prospect of advantage.

Mr. Lynn's method being liable to exactly the same practical objections as Mr. Yarrow's, any additional remarks upon it would be superfluous.

I am, sir, your obedient servant,

Greenwich Hospital, Oct. 14, 1826. E. Riddle.

XLIII. *Chemical Researches on Starch, and the different amylaceous Substances of Commerce.* By M. J. B. Caventou.*

(*Read at the Royal Academy of Medicine of Paris.*)

MORE than eight years since, I undertook an investigation of the various amylaceous substances which are objects of commerce, in which they are designated by the names of *salep*, *sago*, *tapioca*, and *arrow-root*. I gave at that period an account of my first researches to the Society of Pharmacy, and was disinclined to publish them, because several of my results did not appear to me to present the general characters which I sought. I was then not aware of the alterations and adulterations which these substances are made to undergo in commerce. Besides, I intended to attempt some new researches on starch or fecula, whose chemical characters, under certain circumstances, did not appear to me to be sufficiently well established, and even seemed quite unknown.

In 1822 I attempted some new experiments; and found some rather interesting facts, which I entered in my memorandum-book, without making any other use of them, because I always hoped to be able to complete the train of research which I had in view. Circumstances had in some degree made me forget

* From the *Annales de Chimie et de Physique*, tom. xxxi. p. 337.

forget the results of these first experiments, when very recently M. Edwards showed me a memoir on fecula, published by M. Raspail, and inserted in a number (for December 1825) of the Annals of Natural Sciences. I must own that the reading of this memoir interested me strongly, by the curious facts which it contains: but as I have observed some which the author has not mentioned, and as we morever differ in the manner of explaining the phænomena, I thought that perhaps it would not be useless to publish my investigations, incomplete as they are.

Action of Water on Starch.

I must premise, in detailing my experiments, that I have always conducted them, and considered the phænomena, under the persuasion that fecula is an immediate, pure, and homogeneous principle. This consideration it seems to me to be important to remark.

It is well known that cold water has no sensible action on starch; but that when this fluid is elevated to a temperature of from 140° to 160° Fahr., it dissolves this principle, and forms a transparent gelatinous mass, which is generally known by the name of *empois*. What then is *empois?* It is, we have long been told, the solution or combination of starch with a certain quantity of water; it is in fact a hydrate of starch. Such is, I believe, the opinion given in all the books on the subject of the nature of *empois*. Nevertheless, if we well consider the properties of this preparation, it is easy to convince ourselves that it evidently differs from starch; or rather that the starch has lost, in this supposed combination, its most characteristic property; that is to say, its insolubility in cold water. In fact, when we have transformed starch into *empois*, it is impossible to obtain this latter such as it was before the experiment; it redissolves in a greater or less degree in cold water, a property which pure starch has not. This result seems then to prove that this principle changes its nature, in its conversion into *empois*, by the action of boiling water, and that the latter is not a simple hydrate.

Of the Empois *of Starch.*

I distinguish two kinds of *empois*: 1st, that at the minimum of starch, which is quite transparent or very slightly opalescent; and 2dly, that at the maximum of starch, which is nearly or quite opaque.

The first, well cooled, becomes diluted and dissolved in a great quantity of cold water; it only leaves undissolved a small white residue, which is starch; the filtered liquor is limpid and

and clear; by evaporation it furnishes yellowish transparent laminæ, which dissolve in cold water without leaving any residue: although this solution becomes of a beautiful blue by iodine, it is precipitated by the subacetate of lead, and by nutgalls, all properties heretofore attributed to the solution of starch in boiling water; the vegetable matter which it contains always differs from this last, since it redissolves in cold. According to this very evident fact, it appears to me very rational to conclude, that the action of boiling water on starch modifies its nature, since it renders it soluble in cold water. But how does the boiling water act here? Is it simply by the caloric which it contains, that the modification of the fecula is produced? Does its dissolving property here go for nothing?

If the temperature of boiling water sufficed to produce such a result, we should equally obtain it by keeping the starch continually exposed for a long time to this heat: but experiment proves the contrary.

Whereas if the temperature is elevated to 212° or a little higher, that is to say to a degree very near to that at which the starch becomes decomposed, immediately this substance takes a slight reddish tint, developes a smell of baked bread; and if then it is left to cool, and exposed to the action of cold water, it dissolves in it; and the liquor which results from it possesses all the characters of that which has been already treated of.

Thus the action of the water is evident. It in some sort stands in the place of an excess of temperature, by reason of its dissolving property, which facilitates and determines a new disposition in the constituent molecules of the fecula.

When the temperature is still more elevated than the former, and fit to subject the starch to a strong torrefaction, this principle is then completely altered; it dissolves with great facility in water, and instead of a blue colour it takes a purple colour with iodine, as results from experiments published at different periods by MM. Bouillon-Lagrange, Dœbereiner, and Lassaigne.

The second kind of *empois* is of a nature and composition similar to the former: it only differs from it by a greater quantity of pure starch which is found in suspension, or in combination, which produces the opacity and the great consistence of this *empois*. Also this compound, treated with cold water, leaves a residue much more considerable than the other.

According to these facts, then, the *empois* is a ternary compound of pure starch, of modified starch, and of water. The presence of these three bodies is indispensable in order to form good *empois*. Modified starch is insufficient of itself to produce

produce this compound. This is so true, that if starch is boiled for a long while in water, care being taken to renew the latter as it evaporates, in the manner Vogel has done, we finish by obtaining by means of evaporation, instead of *empois*, a hard, horny, transparent matter, which dissolves in cold water, and in which no sensible traces of pure starch are to be found. It is easy to conclude, from what precedes, that in this long ebullition the water has had time to react upon the whole mass of starch, and to modify even the last particles of it.

This starch, thus modified, has already been perceived by chemists, but it has never been made the object of a particular study; they have always been contented with saying, that starch, after having been dissolved in boiling water, partly redissolved in cold water.

It is only of late that M. de Saussure has designated it specifically by the name of *amidine*, considering it as a product of the spontaneous decomposition of the *empois*.

I do not, however, believe, that the origin of amidine is essentially the result of a spontaneous decomposition. I am persuaded, on the contrary, that this sort of putridity of *empois* is quite foreign to the production of this substance; and it is sufficient to cast a glance on the process by which M. de Saussure extracts amidine, in order to be convinced of it. In fact, this philosopher obtains amidine by boiling the residue insoluble in cold water, of *empois* decomposed by time; filtering the liquors after cooling, and bringing these near to dryness, he obtains a fragile, yellow, and half transparent matter, which is amidine. If now we recollect the nature of *empois*, such as we have just considered it, is it not evident that its insoluble residue left to grow sour must be, for the greater part, formed of pure starch, which, by its state of aggregation, must have been the last principle liable to be attacked by putridity? Is it not also evident, that by treating this amylaceous residue by boiling water, M. de Saussure has modified, by this very act, the nature of this starch, and has converted it into amidine? I am so much the more induced to believe it, as this amidine has all the characters of our modified starch; it becomes blue by iodine, precipitates infusion of galls, &c. Thus, according to my view, M. de Saussure, far from having extracted amidine, will have formed it himself; and he will thus have misunderstood this truth, of which he does not speak in his Memoir, "that in the ordinary processes of our laboratories we form, and often change, the nature of the bodies which we wish to study."

We have just seen that the essential characters of amidine

are,

are, its solubility in cold water, and the property of acquiring a blue colour by iodine. We have also proved that it may be obtained in two ways; either by the action of boiling water on starch, or by that of a higher temperature to which we might immediately expose this substance. We have seen by these two methods, which lead to the same result, how much a very weak chemical agent may sometimes stand in the place of the excess of another much more energetic, especially with regard to an organic body whose elements are very soft.—We will now pursue the study of the phænomena.

If we boil for a long time an aqueous solution of amidine, it at length loses the faculty of becoming blue by iodine, although it still preserves that of precipitating by galls and the acetate of lead; it takes with iodine a purplish colour: then the amidine has changed its nature, and is become much more soluble in water: starch or fecula may be immediately brought to the same state, either by a pretty strong torrefaction, or, as I have effected it, by putting this vegetable principle when heated, in contact with sulphuric acid diluted with twelve times its weight of water: there is an instantaneous solution, and the liquor carried to ebullition, then cooled, takes a purplish colour with iodine, and is no longer precipitated by water. If the ebullition is continued longer, the iodine no longer produces any phænomenon of sensible coloration. I am ignorant whether by an immediate torrefaction of starch, continued with skill, we should obtain a gummy matter which would not become purple with iodine, as in the preceding cases,—I am inclined to answer in the affirmative.

A part of these phænomena is again produced by *empois* decomposed by time. Perhaps time is not sufficiently considered, which nevertheless presents in many cases a valuable chemical agent. I took some *empois*, not with basis of fecula of potatoes, but of fecula of wheat; I left it to itself during more than six weeks, in the heat of the summer it became sour: diluted in water in this state and thrown on a filter, the following phænomena are observed. If iodine be poured upon the liquor, without colour as it was, it becomes a fine purple, whilst the insoluble matter left on the filter takes at the same instant by the same re-agent a beautiful blue colour. This decided result cannot take place, unless all the amidine has been decomposed into *empois*, and has passed either into the state of gum or into that of sugar. Although I have not verified it, analogy induces me to think that the development of the purplish colour is only owing to the sort of gum formed in this case by the acescence, and that the sugar has no agency in the phænomenon.

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These results make me think that iodine is really susceptible of forming a combination with starch; and what in my mind supports this opinion is the following fact. If, to the above liquor filtered and brought to a *purplish* colour by iodine, a little amidine or starch be added, at the same instant these two bodies take away the iodine from the substance which causes the purplish colour, and produce a *blue* combination, which remains dissolved if produced by the amidine, and which becomes precipitated if it is owing to the starch. If this last combination is isolated by the filter, we may again successively reproduce, as above, the purple and blue colours.—Does not this fact prove that there is really a chemical action between iodine and starch, and that the colouring is not determined merely by a physical effect, as M. Raspail has advanced?—when there is a very evident affinity, can we call in question the chemical action?

Application of the preceding observations to the investigation of the amylaceous substances of commerce.

As I have said at the beginning of this memoir, I had at first begun at once the study of salep, of sago, of tapioca, and of arrow-root; and the chemical phænomena which I had observed with these substances had induced me to admit sago and tapioca especially, as new species of starch. But I had made my experiments under the notion that fecula was perfectly known to us in all its chemical properties; and it is this which made me deduce from my results a consequence in a certain degree premature: but the facts which I published at this period will remain altogether the same; I shall only differ in the way of considering and explaining them.

Of Salep.

(This article is extracted literally from my notes, I have not altered any thing in them.)

Salep reduced to powder and put in contact with cold water becomes easily diluted by agitation, and forms a sort of half-liquid and translucid *empois*. This *empois*, diluted in a sufficient quantity of cold water and thrown on a filtre, gives a transparent gummy liquid of a saltish taste. There remains on the filtre a gelatinous trembling matter, insoluble in water, whether hot or cold, but which augments considerably in volume in this liquid. This gelatinous substance, separated from all the soluble principle, first by cold water and then by boiling water, was set aside to be examined.—It will be spoken of hereafter.

Aqueous

Aqueous maceratum *of Salep.*

This *maceratum*, tried by some re-agents, acts in the following manner: it precipitates by nitrate of silver and oxalate of ammonia; corrosive sublimate makes in it a slight cloud; common acetate of lead produces no precipitate in it except when the liquor is very concentrated, but the sub-acetate of lead gives a very abundant precipitate.

This liquor diminished by evaporation, leaves a viscous matter which has a very great analogy with gum: it does not change by iodine, it is precipitated in the form of white flakes by alcohol; but it presents this small difference, that it does not easily dissolve again in a pretty large dose of aqua-fortis.

A portion of this matter calcined in a platina crucible, leaves a residue from which cold water separates sea salt, whilst acidulated water totally dissolves some remaining phosphate of lime.

The presence of sea salt in salep, if it be not accidental, is rather remarkable; for in general this salt has only been found in sea-plants, and I do not think that salep is similarly circumstanced with these: as to the phosphate of lime which is dissolved in a liquid not acid, we should have reason to be astonished at it, if there did not exist in this liquid a certain quantity of viscous matter which holds the calcareous salt, and retains it in solution. It is also known that M. Vauquelin, in his analysis of rice, has announced a similar fact, but which presents this difference, that instead of gum, he made his experiments with starch; and even to dissolve a sensible quantity of phosphate of lime, he was obliged to expose the mixture to heat, for cold water did not act on it. Thus it is very certain that phosphate of lime dissolves in water, not only with the assistance of acids, but also by the help of starch, which receives the action of boiling water, and of a gum similar to that existing in salep. It is very probable that we shall hereafter find other vegetable matters that will also partake of this property, which indeed is common to several animal-matters.

Aqueous Decoction of Salep first treated by cold water.

Salep purified as much as possible by cold water, and then treated with boiling water, leaves in this fluid a small quantity of a matter which gives it an opaline appearance. This liquor filtered and tried by iodine, becomes of a beautiful blue, and precipitates at the end of some hours iodide of starch.

I found nothing else in this decoction, and had reason to be astonished at the little starch which existed in it, because salep was generally regarded as nearly pure amylaceous fecula.

There remained nevertheless a great quantity of transparent gelatinous substance, which had become considerably swelled. This substance no longer gave out any thing to the water, dissolved easily in hydrochloric acid, gave oxalic acid by nitric acid, and possessed in short all the properties recognised in bassorine.

100 parts of salep lose by calcination 96 parts: it then contains 4 per cent of fixed principles, composed of sea-salt, of phosphate of lime, and of some traces of sulphate of lime.

Thus salep is composed of three bodies already known, the respective quantities of which may be stated in this manner: A *little* gum, *very little* starch, and a *great deal* of bassorine. According to what has been said, we see that the rank which is generally given to salep amongst amylaceous matters may be much disputed. This compound might on the contrary be very well placed by the side of gum tragacanth; for, according to Bucholz, this gum is formed in a similar manner. It contains indeed a gummy part soluble in cold water, and a matter which swells considerably in it, nevertheless without dissolving; yet this differs from the bassorine of salep, in as much as that dissolves by heat, and loses by this circumstance its most remarkable property, that is to say its insolubility and sponginess in cold water. I shall not take into consideration the starch which is found in salep, and which does not exist in gum tragacanth, because it is contained in such small quantity that it might strictly be considered there as accidental. In these latter times, they have nevertheless proved the presence of starch in some specimens of gum tragacanth.

This analytical examination of salep proves then that its nutritive virtue is not owing to starch. Nevertheless I should say that all the orchis roots are not of the same nature as salep; M. Vauquelin told me he had extracted from some indigenous orchides a considerable quantity of fine starch, whilst M. Robiquet assured me that he could not extract any trace of the same principle from some of the orchides of our country. These contradictory results prove how inconstant is the presence of this amylaceous principle in these roots, and can have very little influence on their medical properties.

For the purpose of throwing some light upon the question, I will here shortly relate a process which was pointed out by M. Matthieu of Dombasle (*Ann. de Chim. t.* lxxvii), for preparing salep with indigenous orchides. The author has made his experiments with the *orchis mascula*, *pyramidalis*, *latifolia*, and *masulata*. He clears these bulbs with care from the little roots and the germ, then throws them into fresh water, where

he washes them; when they are well cleaned, he threads them in the form of chaplets, and boils them in plenty of water until some bulbs dissolve into mucilage, which requires in general twenty or thirty minutes. If the ebullition has not been continued long enough, the salep retains a very strong and disagreeable taste: when the process is complete, it is either dried in the sun, or in a stove.

Not having had an opportunity of examining indigenous orchides, I could not establish a comparison between these bulbs and the salep of the East; but I shall remark that the treatment to which M. Matthieu of Dombasle has subjected the indigenous salep, is sufficient to indicate a great analogy of composition between these two bodies. It evidently proves that the indigenous salep is not richer in starch than exotic salep, and that this principle is not in these bodies the base of their nourishing properties. Besides, M. Matthieu says himself, that the orchides submitted to his experiments are in great part formed of a mucilaginous matter analogous to gum tragacanth.

Of Sago.

A certain quantity of sago in well-chosen grains and reduced to powder was put into maceration with cold water. At the end of twenty-four hours, the opaline liquor, a little mucilaginous, was filtered, and passed very clear. This liquid acted with alcohol and nitric acid in a manner similar to that obtained with salep; it was precipitated by the subacetate of lead; subjected to evaporation, this fluid deposited transparent scales, which treated by nitric acid did not give any trace of saccholactic acid.

Thus far these properties of sago have nothing decisive, and would allow us to consider the matter dissolved by cold water as of a nearly gummy nature; for many gums give no mucic acid by the action of nitric acid; but what characterizes it in a very remarkable manner, is, that being put in contact with iodine it takes a magnificent blue colour, and forms a soluble iodide in cold water, but which in other respects acts by heat like the iodide of starch.

Can the presence of starch be admitted in the liquor where this matter has been dissolved, the only substance known till now to become blue by iodine? This is not probable; for starch is quite insoluble in cold water, unless by the assistance of an acid, and our liquor is neutral! Neither can it be thought that some very diluted starch has been carried along and has passed through the pores of the filtre; for I had the precaution to filtre the liquors through three filtres, of which one had

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been made with a paper twice doubled. Should we like better to suppose that a gummy matter has facilitated the solution of a little starch, as it did with the phosphate of lime in the salep? I shall then ask why this phænomenon was not manifested by the salep itself. Besides, artificial mixtures of great quantities of gum, either tragacanth or arabic, and of a very little starch, put to steep in water and filtered at the end of twenty-four hours, give a liquor purely gummy, and in which iodine does not indicate the smallest trace of starch. We must then necessarily conclude that sago is formed of a variety of starch quite peculiar, which may be characterized by its solubility in cold water, and distinguished from gum by the action of iodine.

Sago, submitted for the first time to the action of cold water by macerating for four-and-twenty hours, swelled considerably and remained at the bottom of the liquid; treated a second and a third time and many times repeatedly by water at the same temperature, this fluid always dissolved sensible quantities of it, which presented the same phænomena as at the first time. Submitted at last to the action of boiling water, the sago totally dissolved, with the exception of a few filaments; and the solution presented the same characters as that made in the cold, only they were more marked. These results indicate, then, that sago is homogeneous in its composition, and that this consists simply in a variety of starch soluble in the cold, and more soluble by heat.

Of Tapioca.

After sago, tapioca naturally succeeds, a sort of fecula, very white, in irregular grains, and which is regarded as the fecula of manioc, freed from the acrid substance which accompanies it by means of numerous washings in cold water, and a slight torrefaction on iron plates.

This substance treated with cold water, swells and partly dissolves; the liquor filtered presents all the characters of that obtained from sago. The often repeated washings always produced liquors which became blue by iodine, and they may be thus continued until the substance is totally dissolved. It is useless to leave the water a long time in contact with the tapioca to obtain the liquor of which we are speaking, for a few seconds suffice. These phænomena indicate then a very great chemical analogy, I would even say identity, between sago and tapioca; and physicians might, it seems to me, substitute the one for the other without inconvenience.

Now if I were asked whether any starch, properly so called, exists in sago and tapioca, I would reply No: 1st, because these two

two substances may be totally dissolved in cold water: 2dly, because they form with iodine blue combinations soluble in the same liquid; and 3dly, because they do not form *empois* with boiling water, as the starches of corn and of potatoes do.

Of Arrow-root.

This substance is brought to us very probably after having been extracted and dried, in the same manner as the fecula of the potatoe is treated here. I shall say very little of it, because it acts chemically like this last. It yields but a trace of gummy matter to cold water, and remains insoluble; whilst boiling water dissolves it like ordinary starch, and converts it into amidine.

[To be continued.]

XLIV. *Notices respecting New Books.*

A Letter to Sir Gilbert Blane, Bart., Physician to the King, from Sir Anthony Carlisle, Surgeon Extraordinary to His Majesty, on Blisters, Rubefacients, and Escharotics, describing the employment of an Instrument adapted to effect those several purposes. London, 1826, 12mo.

FEW persons need be reminded of the tediousness and troublesome operation of blisters, as employed by the physician to relieve internal affections of various kinds, or to cause a translation of inflammatory action from an inward to a more superficial part of the body,—as to the skin; but many will be glad to learn, that the numerous inconveniences attending their use may now be obviated by the employment of an instrument which will effect as much in a few moments as blisters can in as many hours. A most unpleasant affection of the urinary organs sometimes follows the application of cantharides, in the form of an ointment, to the skin; for when a small portion only is absorbed into the system, a painful strangury is frequently produced: and when we consider how inadmissible this application must be in all inflammatory or irritable affections of the urinary organs, and how tardy their operation in those acute cases where an immediate effect is most desirable, we feel anxious for the success of the method now first proposed to the profession by Sir A. Carlisle. This gentleman assures us that this method of blistering is free from cruelty, and, with common caution, not liable to those evil consequences which are sometimes attendant on the ordinary methods. "The action of a Metallic Substance," he says, "heated in boiling water, was first tried on my own arm; and although the instant pain was severe, the sum of distress was far below that which is occasioned by a blister of cantharides.

cantharides. I have since employed the Blisterer upon many patients, both women and men, selecting those who had lately been blistered with the cantharides plaster; and they affirm, that the momentary endurance of the instrument is preferable to the former method; and where a repetition of blistering has since been required, those patients ask for the quick process."

This method of blistering consists in applying the metallic instrument, heated to the temperature of 212° by immersing the head of it in boiling water for five minutes, to the part intended to be blistered, previously covering the part with a piece of silk moistened with warm water; the instrument requires to be gently pressed against the part, and if the blisterer has not been longer than five minutes out of the boiling water, the pressure need not be continued more than three or four seconds. The first effect is a corrugation and paleness of the skin; the red blood soon returning, an inflamed redness appears, which gradually proceeds to discharge a fluid (the serum) and to detach the cuticle. After wiping the part dry it is to be covered with soft dry linen, and when the process is completed, it must be dressed with Kirkland's Neutral Cerate, this being preferable to soft greasy ointments.

Applying the instrument immediately at its highest temperature, and holding it firmly on the part ten seconds, it will produce an *escharotic effect*, and the size of the eschar may be determined by applying one or other of the surfaces of the instrument. If a *rubefacient effect* only is required, the part must then be covered with *dry* silk, and the instrument moved slowly over the surface, until a sufficient degree of pain and redness arise. The metallic head of the instrument will retain a blistering temperature for nearly fifteen minutes, whilst its ready transmission of heat gives it more potency than water or any slower conducting substance. Sir A. Carlisle wishes the application of it to be restricted to medical men; but we fear they will find great difficulty in obtaining the consent of their patients.

Its application to the palm or back of the hand or the soles of the feet, or to the wrists or ancles or upper part of the foot, is inadmissible, because of their exposed bones, tendons, and ligaments.—Were we to offer any objections to its use, they would be merely conjectural; but we will venture to ask, whether the slow action of the common blister is not more likely to extend its influence deeper than the quick process here recommended? and whether, as the body is obnoxious to severe and sudden pain, bad consequences might not sometimes follow from a sudden burn, as this in fact is?

The instrument is sold by Messrs. Stodart, 401, Strand.

XLV. *Pro-*

XLV. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES OF PARIS.

June 12.—THE Rev. Mr. Bremner, a Scottish clergyman, communicated a memoir on magnetism.—M. Solier presented a plan of the experiments he has instituted with the object of determining the action of the sun on colourless flowers.—M. Latreille presented some specimens of *Cardium edule*, found in a recent alluvium at Abbeville, at the depth of about twenty-three feet, and at the distance of four leagues from the sea in which these animals live.—M. Michelot announced that M. Billaudel, civil engineer at Bordeaux, had discovered in a quarry on the banks of the Garonne, a cavern in which he found a quantity of the bones of various animals; among them the jaws of the hyæna, of the lion or the tiger, and of the badger, bones of the ox, &c.—M. Chevreul terminated the reading of his paper entitled Chemical researches on dyeing; application of indigo and prussian blue to silk.—Dr. Murphy presented a manuscript work, entitled A Dissertation on the affinity which exists between the phænomena of the tides and the temperature of the atmosphere.—M. Turpin read a memoir, entitled *Organographie végétale:* observations on some microscopic vegetables, and on the function which their analogues enjoy in the formation of the cellular tissue.

June 19.—M. Magendie read a note on the direct application of galvanism to the nerves of the orbit, and on the employment of this method in the treatment of amaurosis.—MM. Thenard and Blainville gave a favourable report on M. Laurent's new method of drawing on stone.—M. Dupetit-Thouars read a memoir on the green colour of vegetables.—M. Daubree read some observations on the deterioration of the colour of prussian blue, called (in dyeing) Raimond blue.

June 26. — M. Dupetit-Thouars announced his intended work on the history of the Orchidean plants collected in the Isles of France, Bourbon, and Madagascar.—M. Collard, of Martigny, read a memoir, entitled On the action of carbonic acid gas on the animal œconomy.

July 3.—M. William Brandes presented a copy of his work, entitled *De repentinis variationibus impressione atmospheræ observatis:* it was accompanied with a letter to the Academy explaining the object of his researches, and requesting the communication of documents suitable for the completion of his undertaking. M. Arago mentioned the results of various barometrical observations, in relation to this request.—The

Academy

Academy received Professor Simonof's memoir on the cause of the difference of temperature between the two hemispheres of the terrestrial globe, founded on some thermometrical observations made by the author during his voyage round the world. (Casan, 1825.)—M. Arago announced several results of his new researches on the influence of the most different substances on the motions of the magnetic needle.—M. Poisson announced that he had arranged a memoir on the theory of questions of this kind, which he would submit at the next sitting.—M. Bérard, in the name of M. Ballart, of Montpellier, read a memoir on a particular substance contained in sea-water, which he calls muride.—M. Raspail read a memoir on hordeïne and gluten, and on the difficulty of isolating the various principles of which any farina consists; with notes on stearine, sago, and adorganthine.—M. Magendie presented a memoir by M. Housset, of Bordeaux, entitled Observations on common honey.

July 10.—M. Poisson read his memoir on the theory of magnetic motions.

July 17.—The minister of the interior transmitted a fragment of an aërolite which fell lately in the environs of Castres (Tarn). It was referred for examination to MM. Vauquelin and Thenard, and the minister was solicited to obtain all the particulars regarding it that could be collected.—M. de Humboldt announced the discovery, by M. Boussingault, of the true geological position of platina. Hitherto this metal had been found only in the alluvial districts of Choco, of Brasil, and of the Urals; but M. Boussingault has discovered rounded grains of platina mingled with those of native gold, in the gangue of veins in the province of Antioquia. These veins traverse a formation of greenstone, diorite, and syenite.—M. Dupetit-Thouars read the first part of his researches on the parts of vegetables which should be denominated organs.—M. Paravey read a memoir on the common origin of the cyphers and letters made use of by different nations.

July 24.—M. Raymond, clock-maker, read a memoir on a new system of balancing without compensation, applicable to clocks, and particularly to the uniform measure of time.—M. Moreau de Jonnès read two notes; 1. Statistic sketches of the extent and value of the cotton trade, and of the fabrication of tissues of that material, and the consumption of them in the principal countries of Europe; 2. On an earthquake at Martinique, in the night of the 1st and 2d of last May.

July 31.—M. Saint-André, professor of therapeutics and materia medica at Toulouse, communicated a memoir on some new products of the analysis of various quinquinas, selected

lected from the best officinal species.—M. Moreau de Jonnès communicated some details on the recent appearance of the yellow-fever in the West Indies.—M. Perrin communicated a manuscript, entitled A Steganographic vocabulary, or the art of communicating rapidly during the day or the night, at great distances.—M. Arago presented the results of barometric observations and measurements made at la Chapelle, by M. Breaute, from 1819 to 1825.—M. Savary read a memoir on the magnetic phænomena produced by electric currents.

MEETING AT DRESDEN, OF THE PROFESSORS OF MEDICINE AND THE PHYSICAL SCIENCES IN GERMANY.

The following article occupies a considerably larger space than we usually devote to our notices of the proceedings of scientific societies; but it announces the formation of a union, and describes the detail of transactions which are stamped with a character altogether novel to this country—a consideration which gives it more than the ordinary interest of such narratives, and excuses us for transgressing our usual limits. Yet we ought to add, that as here presented, it is only an abridgement of three articles from the *Allgemeine Zeitung*, a journal which ranks high in its department throughout Germany. The passages omitted are of no historic value, and would therefore be of little interest to the English reader.

About five years ago a voluntary union was formed of all the cultivators of the different branches of natural science. The plan agreed upon was, to hold a general assembly annually on the 18th of September, and the four following days, at a place to be appointed by the members, for the purpose of communicating in common any thing worthy of remark, as well as the newest discoveries which each member might have made in his particular branch of inquiry. The Society was to have neither fixed members nor separate journals of its proceedings; its directors were to be temporary; and any one had liberty to enter it who might in any degree have distinguished himself in his own province.

The greater part of those who attended the earlier meetings, delighted with the journey, were naturally induced to attend the succeeding ones, at each of which they had the opportunity of becoming acquainted with distinguished cultivators and friends of science. In this manner the Society has thrived, without ever issuing a diploma, which is too often only the phantom of merit.

The first meeting of the kind was held at Leipsic in 1822; at which, to the great gratification of all present, the venerable Blumenbach attended; and every thing went off in the best manner.

manner. On the following year the friends assembled at Halle, in the hall of the University; at that meeting the fertile Dœbereiner of Jena announced his remarkable investigations respecting platina. The third meeting was held in the lovely vine-clad town of Wurzburg, where the worthy Outrepont gave such active assistance; and the fourth was held, in 1825, at Frankfort-on-the-Maine, where the "Collections" of the Sinkenberg Institute, magnificently enriched by the patriotic Ruppel, afforded a rich treat to the very numerous assembly, and were luminously and eloquently explained by Professor Eretschmer, who also filled the office of Secretary.

At this meeting it was determined that the next should be at Dresden. The director of the Medico-chirurgical Academy in that city, Seiler, was chosen to be the President, and Professor Carus, the Secretary. The picturesque environs of Dresden, the fine harvest-season, the museums and works of art, all contributed to heighten the pleasures of the occasion, and the assembly was attended by upwards of fifty out-resident Professors, Physicians, and men of science, from near and distant parts; and all parted at the conclusion with the most cordial congratulations till the next year.

The members of the two long-established Royal Societies of Medicine, and those of the Royal Wernerian Societies, forming a body of fifty residents at Dresden, undertook to give a hospitable reception to the whole.

Counsellor Seiler, who, with his colleague Carus, most efficiently conducted all the arrangements, had previously procured the requisite subscriptions, and provided what seemed necessary for the occasion. As the meeting was bound to be held with open doors and to afford admission to the curious, none of the academic halls seemed sufficiently spacious for the friends actually present and the additional influx of visitors which was expected; the superintendent, Von Watzdorf, graciously offered the great hall of the States-Assembly in the domestic palace, where, besides the triple row of benches ranged around a round table of considerable size, there was still room for some hundreds of spectators; and the number even of these on the six days when the prælections and communications lasted from nine in the morning till one, amounted to 400. Along all the walls and in the recesses of the windows stood large pots of exotic plants and shrubs brought from the royal botanic gardens, forming by tasteful arrangement a lovely temple of Nature. An adjoining room was open for particular demonstrations and exhibitions, which were to be seen at the conclusion of the sitting,—the requisite limitation. The full-length portrait of the king which decorates the hall seemed to glance

approbation

approbation on the assembled circle, and indeed, the monarch, himself a real lover of science, had ordered that all the royal museums and collections should be open during this period to all the visiting men of science, who also had tickets of admission, of which, both early in the morning and in the afternoon, the most diligent use was made. One of the most spacious restaurateur-establishments at the top of the old town was specially engaged, to be constantly open as a point of union, and was supplied with suitable hospitality, of which the visitors and many of the residents very agreeably partook.

In the middle of the week a splendid entertainment and feast was given on the smiling shore of the Elbe to the visitors and other friends of science. Ornamented gondolas, in the front of which was a band of music, received the guests at the bridge, and on their return they were greeted by a fire from small cannon and music of a second band. The animated view of the stream and of the amphitheatre of vine-clad hills delighted all; and the inspiration of the scene was heightened by toasts and songs, and all the reciprocations of cordiality. Among the guests were many ministers of state, literati, poets, and artists of Dresden: in this pleasing variety, however, the leading object of the occasion was not forgotten; but on the contrary was promoted with unexampled activity. So numerous were the essays and other communications which, for the most part, were delivered without manuscript, that many deemed it better to withhold what they had intended to deliver, and had the time been twice as long there would have been ample materials to fill it. No branch of medical or natural science was passed over without being elucidated by fresh information, or receiving some additional confirmation. Physiology united with chemistry on the one hand, and with comparative anatomy through the whole range of organic nature on the other. Discourses on all subjects were delivered;—on those included in the wide range of the vegetable kingdom by Treviranus of Breslau, and Reichenbach of Dresden. In mineralogy, Count Caspar Sternberg, the magnanimous promoter of the study of nature, delivered one on petrified ferns, palms, and flowers, with references to his splendid work; Counsellor Cotta of Tharand, on the volcanic phænomena of the Kammerbuhl at Eger; Professor Breithaupt of Freiberg, on some of the newest minerals. In zoology, Professor Eretschmar of Frankfort-on-the-Maine, delivered a discourse on the African dogs and giraffes, which were first brought to notice by Ruppels, and now appear in separate copper-plates; and at the same time the Professor gave critical notices of the latest prælections of Lichtenstein of Berlin

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concerning antelopes. Besides some beautiful notices by Otto concerning several new facts in comparative anatomy, Counsellor Seiler described his recent anatomical investigations; Professor Munz of Landshut described the splanchnology composed by himself and illustrated with his own lithographic drawings. Professor Husche of Jena, and in particular the great master of comparative anatomy, Carus, delivered an account of his grand discovery of the circulation of the blood in insects: and many other anatomical and obstetrical practitioners made communications which even the uninitiated might listen to with profit and delight.

Above all, the modern queen of sciences, chemistry, had her fullest rights; not only in the twofold essay delivered by Professor Lampadius of Freiberg on the medicinal application of carburet of sulphur; in the interesting information given by Dr. Struve, on the composition of his mineral waters; in Dr. Geitner's paper on the application of niccoliferous silver, which is of such great importance to the manufacture of pottery; but also in important communications from Professors Munke, Purkinge, Scheigger, and Runge, on the manner in which quicksilver with brine and sulphate of copper stirred with a small iron rod, forms rotatory crystals and hastens solution; and in the interchange of ideas which was freely indulged in by many other able men in their separate departments.

A learned controversy arose respecting a dissertation by Professor Wilbrand, of the first importance in regard to practical medicine, in which Professor Reich of Berlin with many others expressed their doubts. Professor Rheum's lively essay on the abuse and use of animal magnetism would have called forth similar discussions if time had permitted. Dr. Froxiess of Weimar made some important communications respecting the situation of the fœtus. Many new productions also, which yet were scarcely to be had at the booksellers', were announced and distributed. Professor Weber's essay on the production of the turf leech (blutegel uber torfboden:—*hirudo stagnalis ?*), which now fetches so great a price, had also its turn. Many other Professors abridged their communications, from the pressure of time. It is impossible here to do justice to the various merits which were displayed on this occasion. One disquisition in particular produced an effect on the audience as extraordinary as it was agreeable, through the perspicuity of its style, and the singular felicity with which the subject was illustrated by means only of a few simple lines drawn on the table: this was by Dr. Oken, on the form of the fœtus from the time of conception and the first germ of life,

life, through the various progressions of the threefold organization. Many of the Professors and others present announced themselves as the representatives of particular scientific unions; and it is to be hoped that at future meetings every society will depute one of its members, and thus actively co-operate with the general body. In this spirit, it was resolved by some of the friends of medical science to collect the best pieces from the minor journals of natural history, and to publish them in one work.

Thus also the reduction of Pliny's books of natural history into a separate treatise by Bœttiger, might well suggest the formation of a union for the promotion of the sciences of philology and natural history. As it is the fundamental principle of this society, not to publish its own transactions, but to regard itself as completely dissolved after each meeting, and to avoid as much as possible the appearance of having permanent offices, an account of its sittings will be given only in the form of the protocol usually contained in Oken's Isis, a chronicle which excludes politics altogether, and is entirely dedicated to the sciences.

The conclusion of the assembly on the 23rd of September was truly impressive, when the two deputies Seiler and Carus in a concise and energetic address rose cordially to express the thanks and good wishes of the Dresden members, and presented to every one present a facsimile lithographic list of all who attended this meeting, and a copper-plate of the building of the Medical and Chirurgical Academy, as a memorial.

How easy will it be henceforward, after the example we have just recorded, to establish a second union of the patrons and students of the Archæology of Germany; on which branch of research a first volume has already appeared in a form truly worthy of a national work; such a union might be productive of advantages important to general history. This object, however, may be left to the care of the good genius of Germany and the favouring regards of its rulers.

XLVI. *Intelligence and Miscellaneous Articles.*

OVERLAND ARCTIC EXPEDITION.

We have just read a letter from Dr. Richardson to Dr. Graham, professor of botany, accompanying 27 packages of seeds, many of them new species, and several suspected to belong to new genera. We have much pleasure in publishing the following extract:

"Fort Franklin, Great Bear Lake, Nov. 1825.—We had a quick passage from Liverpool to New York, and from thence we

we have journeyed with little rest and much expedition to this quarter. Captain Franklin reached the Arctic Sea exactly six months from the day he sailed from England, and I have been nearly within sight of the Copper Mountains—a distance which we attained on the former journey only in the third summer. Having been so fortunate as to effect so much in one season, and with our sea stores unimpaired, our prospects are flattering, and we hope, in the course of next summer, to make the world acquainted with a larger portion of the northern shores of this continent than they at present know. We have travelled, however, too quickly for the purposes of natural history; and since we have entered the line of country we formerly travelled through, I have not added more than fifty species of plants to my last. Drummond (late of Forfar), who is in a more fertile country, will, I hope, produce a pretty complete Flora of that quarter. He is most zealous, and for two summers will direct his attention solely to that end. Although this season has been remarkably fine, and the fall unusually late, vegetation ceased with us at the end of August, and we have still eight months of winter to come. In the remaining two months of the year all our collections must be made."—*Morn. Chron. Oct.* 9.

GEOLOGY OF ST. HELENA.

The following particulars on this subject, are extracted from an Address to the Agricultural Society of St. Helena, delivered by General Walker, the Governor, on the 13th of February last.

"All our knowledge of the geology of the island consists of loose, indefinite, and casual observations. The remains of fossil bones, of madrepores, of gravel mixed with lava, of shells and calcareous strata, with siliceous rocks, present the most interesting, and, at the same time, the most dissimilar appearances. There are perhaps no primitive formations. A description of the geology of St. Helena, accompanied by a geological chart, would be highly useful and valuable.

"The soil, rocks, and plants must be deeply interesting to the chemist and the man of science; he may contrast the black and unproductive lava, with meadows covered with grass: he may see the singular and interesting spectacle of the layers or strata of different masses along the coast. In the interior he may observe the appearance of caverns and grottoes which suggest the volcanic origin of their formation; they disclose the contention which prevailed at their birth, and indicate the terrible blaze which attended probably the production of this island. This suggests a question of importance. Are we to view the island as originally thrown up by fire, or as a por-

a portion of an archipelago, which was destroyed by volcanic fires, leaving alone this fragment?

"After this passage was written I became acquainted with Mr. Andrew Blaxam, a very ingenious young gentleman, who accompanied the Right Hon. Lord Byron in His Majesty's ship *Blonde* on a mission to Valparaiso and the Sandwich Islands. Warmed by a love of science, and peculiarly devoted to geological studies, at my request Mr. Blaxam drew up a memorandum of the remarks which he was able to make during such a cursory inspection as his short stay permitted. I have much pleasure in reading these remarks to the society, as they are valuable, and may be a foundation for any future geological account of this remarkably formed island.

"'The island of St. Helena is peculiarly situated in the South Atlantic; and, like other islands in the same ocean, is entirely volcanic. The external appearance of it presents a forbidding and dreary aspect, on account of the rugged and steep cliffs of lava that surround the island, and form a barrier to the waves. Many of these are regularly stratified by several successive deposits of volcanic matter, and in some places veins of a red sandy appearance are visible. In the interior of the island, the valleys and ridges are composed of basaltic lava in its most compact state, together with cellular and other varieties, and indeed in almost every stage of decomposition. Obsidian or pumice-stone has never, I believe, been found here.

"'All the valleys and intermediate ridges appear to concentrate in *one large basin* on the south side of the island, which is evidently a part of the volcanic crater from whence this insulated mass has been formed. The ridge termed Diana's Peak (the highest spot in the island), forms one edge of this crater: it is entirely composed of lava, but the greater part of it being in a high state of decomposition, possesses great depth of vegetable soil, and is necessarily one of the most fertile spots upon the island: here the indigenous cabbage-tree particularly flourishes; here also are found brambles, with a variety of ferns and other plants, together with shrubs and trees. This ridge, as it approaches the sea, inclines towards it; so that we may suppose the remaining ridge, which is wanting to form the edge of the complete crater, lies buried in the sea.

"'The spot known by the name of Sandy Bay, and the fantastical and abrupt pointed rocks in the immediate neighbourhood of that place, have, without doubt, suffered much from volcanic agency; and it is a curious fact, that a calcareous earth, or lime-stone, is found there from which good lime is procured and burnt, sufficient for all purposes of masonry.

"'The

"'The volcanic matter and lava appear to have flowed from this immense crater in every direction, and to have formed the ravines, valleys, and intersecting ridges of the island. The edge of the crater, from Diana's Peak round to the opposite side, is accurately and well defined throughout. There is no appearance of any other crater in the island except this; and from its extent and great depth, when in action must have been very powerful and terrific.

"'In cutting away the lava at Ladder Hill, many feet below the surface, small bones have been found, apparently about the size of a rat's, and more particularly a small rib bone entirely covered with an incrustation of stalagmite. In what manner these have been originally introduced must ever remain a mystery, and will always afford a curious subject for investigation and research: there is but one probable mode of accounting for it,—on the supposition that the animal might have crept into a crevice of the rock and there died; for if a bed of lava in its liquid state had flowed over them, they would probably have been consumed, nor would they have been found incrusted with stalagmite.

"'The large portion of decomposed lava which is found upon the island, together with the vegetable soil formed since the action of the volcano, constitutes in many places a rich mould of sufficient depth for the largest trees to take root and flourish in.

"'It is evident from the present state of the island, that the volcanic fire has ceased at some very remote and unknown period; and there is little probability of its ever breaking out again, as the island itself is small, and the combustible matter appears to be entirely consumed.

"'In some part of the island veins of jasper, commixed with small portions of opal, are seen traversing the volcanic rock: several heavy and irregular-shaped stones, containing a portion of iron, have been found: argillaceous earths are also found, mixed with a fine white and adhesive clay: shells in a state of petrifaction have also been discovered many feet below the sea, in a concretion of pebbles and lava, forming a kind of pudding-stone or breccia.'"—*Asiatic Journal.*

LUMINOUS METEOR.

Edinburgh.—On Sunday, August 27th, about nine o'clock in the evening, a meteor shot over this city, in a direction from S.W. to N.E., which was visible for a few seconds, and brightly illuminated the sky in its track. It resembled a great sky-rocket. *Falkirk.*—Sunday last was marked for the sudden

rise

rise which the thermometer experienced, rendering the atmosphere so sultry that we were reminded of the late great heats, and which was not diminished by the peals of distant thunder that continued to grumble during the afternoon. At quarter to nine o'clock in the evening, one of the grandest celestial phænomena that has occurred in the memory of the oldest person was exhibited. The air was quite calm; but there was a heaviness which indicated a surcharge of electric matter. A vivid glare of light, tingeing every object with a pale blue colour, suddenly blazed forth in the heavens, rendering the minutest object visible as at noon-day. The eyes of every person in the street were instantly directed to the east, where a most sublime sight met their gaze. A large body of fire, in shape like a jargonelle pear, and apparently of the size of a bee-hive, was moving in a direction from SW. to NE. with a rushing noise, something similar to that of a rocket. It left behind it a very long train, not of sparks, but fluid-like, and of the most resplendent prismatic colours. It continued visible for nearly fifteen seconds, having gone over a space of about forty-five degrees, and descended apparently so low that it actually seemed to approach within a hundred feet of the earth. Having assumed a deep crimson tint, it was extinguished without any explosion, several pieces of red matter, like cinders, falling perpendicularly downwards, which were evidently the burnt remains of the nucleus. *St. Andrew's.*— On Sunday evening last, at about a quarter before nine, there was seen in this city (St. Andrew's), a highly luminous meteor to the south-east of the city. When it was first observed, it had the appearance of a comet of transcendent brightness, having a nucleus of about half a degree in diameter. It appeared to shoot forth in a direction from SW. to NE. over a circular path of about 35°, and gradually diminishing in magnitude, until it finally disappeared. What was perhaps most remarkable in this meteor was, that in its orbit it did not present an unbroken volume of light, but appeared to throw out bright sparks in all directions, resembling, in some degree, a sky-rocket. The same appearance was observed at Cupar at the same time. *Bridlington.*—Sunday evening 27th, about nine o'clock, a luminous meteor of dazzling brilliancy was seen at Bridlington, for several seconds, in a NNE. direction; in disappearing, which might be compared to bursting, it presented bright sparklings of a reddish yellow colour. The night was beautifully clear and serene.—*Edinburgh New Philosophical Journal.*

BITBERG METEORIC IRON*.

According to Stromeyer, it contains, iron 81·8; nickel 11·9; cobalt 1·0; manganese 0·2; sulphur 5·1: = 100·0. Stromeyer had not examined it for chrome, but intended to do so.—*Edin. New Phil. Journ.*

MINES OF PLATINUM.

At a meeting of the Academy of Sciences of Paris, held on the 18th July last, Baron Humboldt communicated verbally to the Academy the following interesting information.

M. Boussingault, a celebrated French chemist, has just discovered a mine of platinum at Antioquia in the department of Cundinamarca. Hitherto this precious metal, so valuable in the arts, had only been found in the Urals in Russia, in Brazil, and in the provinces of Choco and Barbacoas, on the coasts of the South Sea, but always in alluvial lands. As this circumstance renders the discovery of M. Boussingault much more interesting, M. Humboldt has been anxious to establish it. He observes, that in all lands where platinum has been discovered, there are found at a very great depth the trunks of trees well preserved. It cannot, therefore, be supposed, that, in this case, transplanted earth has been mistaken for real rocks decomposed *in situ*. With regard to the platinum found in the province of Antioquia by M. Boussingault, there can be no doubt that this metal exists there in real veins in the valley *de Osos*, and it is sufficient to pound the materials which these veins contain, in order to obtain from them, by washing, the gold and the platinum which they contain.

M. Humboldt had not himself visited the country where M. Boussingault has discovered the platinum and gold; but experience has proved to him that almost all the auriferous soils of America belong to the formation of diorite and syenite, and it is in this formation that M. Boussingault has discovered the platinum mixed with gold. The valley *de Osos*, where the platinum occurs in veins, being very near the province of Choco, from which it is separated only by a branch of the Cordillera of the Andes: this circumstance accounts for the presence of the same metal in the alluvial soils of the valley *de Osos*.

M. Humboldt announced at the same time, that mines of platinum had recently been found in the Uralian Mountains, in the government of Perma. These mines are so rich that the price of platinum fell nearly one-third at St. Petersburg. Hence we may reasonably expect that this valuable metal will cease to bear that high price at which it has hitherto been sold. In 1824, the auriferous and platiniferous soil of the

* See Phil. Magazine, vol. lxv. p. 401.

Ural

Ural produced 286 *puds*, which gave 5700 kilogrammes of metal, having a value of nineteen millions 500,000 francs. The mines of all Europe together do not produce annually more than 1300 kilogrammes. Those of Chili yield only 3000, and all Columbia furnishes only 5000.

The Ural yields at present as much gold as was ever obtained from Brazil at the time when its mines were most productive. The maximum, which took place in 1755, was 6000 kilogrammes of gold. At present Brazil yields only 1000.—*Brewster's Journal.*

ON HYALOSIDERITE, BY PROF. BREITHAUPT.

Professor Walchner has given a very accurate mineralogical description, and also a chemical analysis of this mineral *. Its locality is that remarkable hill, named *Kaiserstuhl*, in Baden, which is composed of members of the secondary trap series. At first sight, it might pass for a new mineral; but Professor Walchner communicated to me his doubts as to its being a new species, and remarked, that it was probably only a variety of olivine. On examining some specimens, I found that it bore the same relation to chrysolite that achmite does to augite, viz. having an inferior hardness, and very low lustre in the compact fractured surface. Measurement proved, that *the hyalosiderite is a variety of chrysolite*, but in a state of partial decomposition. This decomposed condition explains the difference in chemical composition from chrysolite. It is worthy of remark, that when a mineral is altered by weathering, that the open cleavages remained but little affected. This is most striking with the felspar family, as in orthoklase, which, when so much decomposed as to be easily pressed into a kind of porcelain earth between the fingers, yet retains its most obvious cleavages. But it is without lustre in the direction of the compact fracture. M. Kühn, inspector of the royal porcelain manufacture at Meissen, has, with an œconomical view, undertaken a chemical examination of the orthoklases, from Aue near Schneeberg. He finds that, in those varieties which are the least decomposed, there is a smaller quantity of potash and more alumina, than in the fresh or unaltered varieties; while the more completely decomposed afford no potash, but more alumina. These examples, we think, are sufficient to prove, that a mineral can only be considered as a new species, when it possesses essential differences from all known species, and can be examined in a fresh state. It also leads to erroneous views as to composition, if the mineral is examined, not in a fresh, but in a decomposed condition.—*Edinburgh New Philosophical Journal.*

* See Phil. Magazine, vol. lxiii. p. 181.

HYPOTHESIS REGARDING MAGNETISM. BY DR. BÜCHNER.

"There are still," says Dr. Büchner, "so many obscure things in the phænomena of magnetism, that it would be rash to present any explanation of these phænomena, otherwise than as a mere hypothesis. We may admit as demonstrated, that the magnetic influences are as extensive in their operation as light, caloric, and electricity, but that they are in a state of reciprocal neutralization, which prevents their being made sensible. There is but a small number of bodies which have the property of breaking this state of equilibrium, and manifesting north and south polarities. Among these we distinguish the loadstone, iron, steel, nickel, cobalt, &c. To what is this remarkable property owing? Is it to a peculiar crystallization of these bodies, or rather to some defect of equilibrium in their chemical constitution? Of this we are ignorant. It seems to me, that it may be admitted, that, as light emanates from the sun toward the earth, magnetism in return emanates from the earth toward the sun, in a state of neutralization in the equatorial zone, which receives the greatest quantity of light, and in a state of polarization toward the poles of the globe, which receive the least of it. It cannot be refused to admit, that light, caloric, electricity and magnetism, are in a certain mutual relation of causality: the question is merely, what is this relation? The following hypothesis appears to me the most simple and most natural.

"The planets receive from the sun light and electricity in the neutral state; they decompose these principles, and reproduce, in their turn, caloric, and the two polarized electric principles. But caloric dilates bodies, and breaks in them the equilibrium of their cohesion, and of their chemical constitution. Then caloric itself undergoes a modification, which is still enigmatical to us, in virtue of which it is transformed into magnetism. All ponderable bodies are conductors of magnetism, for which they appear to have little affinity. Organized and living bodies, such as our own, are sensible to light and heat; but we want a sense for the magnetism with which we are constantly surrounded and penetrated: hence the difficulty of understanding this agent aright. If we inhabited the sun, perhaps, in place of a sense for perceiving light, we should possess a sense for perceiving magnetism.

"In the present hypothesis, magnetism would not emanate from the earth only, but also from all bodies in the universe that are illuminated by the sun. We may consider as proofs of these magnetic emanations; 1st, The magnetic currents which are established in the conducting wire of an electro-chemical apparatus or in a thermo-magnetic metal; for the earth

earth itself, considered in this point of view, is nothing else than a great thermo-magnetic apparatus; and, 2dly, the circumstance that, in the most elevated regions of the earth's atmosphere which man has hitherto been able to attain, the magnetic needle remains as strongly polarized as at the very surface of the globe.

"Further, if we reason according to the ordinary laws of nature, we cannot regard it as probable that the planets, placed as they are right opposite to the sun, act an entirely passive part. We see every where in the universe mutual changes taking place; why should the sun, on its part, be always giving, and never receiving any thing in compensation? If it were so, notwithstanding the magnitude of its mass, the productive power of light which it possesses would necessarily diminish, after a lapse of some thousands of years, while the earth and the other planets would be supersaturated with light and heat. Now, this is what we do not see happening. It appears to me much more probable, that there must prevail, with respect to this, in the planetary system, a continued order and a periodical return. The sun might be considered as the heart of this system; a common principle would emanate from this centre under the form of light, and would flow toward the planets, as the arterial blood flows toward the extremities; it would there be successively transformed into caloric, electricity and magnetism. In this latter state, it would flow back toward the sun, as the venous blood flows back toward the heart, to be reconverted into a state of light, by a modification the inverse of the first. Perhaps mathematicians might even seek the cause of the laws which regulate the motions of the celestial bodies, in this alternate transportation of light toward the planets, and of magnetism toward the sun. We see motion result from analogous currents in the rotatory electro-magnetic apparatus."—*Bibl. Universelle.*

SPONTANEOUS COMBUSTION OF LAMP-BLACK.

We subjoin the following details of an alarming instance of spontaneous combustion on board the *Catherine* (from Portsmouth to Calcutta), which we hope will be useful as a warning to others, to take every precaution when they have got such an apparently dangerous article as lamp-black on board.

Extract from the ship *Catherine's* log, 3d Feb. 1826:—"Lat. 1° 37′ north. long. 86° 55′ east. At 1 P.M. a strong smell of burning and an appearance of smoke, as if rising from the fore-hold, was observed by some of the people between decks: this was immediately reported to the officer on the quarter-deck, in consequence of which the fore-hatches and

and fore-scuttle were taken off, when a suffocating smell of fire and clouds of smoke began to issue from both places. On going into the fore-hold, and clearing away the goods near the hatchway, found that a large cask of lamp-black in the starboard wing had taken fire, and was giving out dense columns of smoke; the cask, although not in a blaze, was too hot to be handled. All the ship's company and passengers were instantly employed in handing down water and wet blankets, the latter being found of the greatest use in stifling the smoke: these enabled the officers and people in the hold, who were indefatigable in their exertions, to remove the surrounding articles, chiefly large jars of linseed and neats'-foot oil, which were immediately hoisted on deck by the prompt assistance of the passengers, and at the same time a constant supply of water passed down the hatchway; and although the people in the hold were frequently driven back by the strong suffocating smell, they at last succeeded in getting the cask, which was on fire and muffled by wet blankets, brought to the hatchway; this was instantly hoisted on deck and thrown overboard, before it had completely ignited or burst into a flame; had it done so in the hold, instant destruction must have inevitably followed, it being surrounded by 200 barrels of tar, and upwards of eighty large jars of oil. As no apparent cause could be assigned for this catastrophe—as no leak either from the deck or from any of the jars could be perceived, and as no light had ever been suffered in the hold since leaving England,—it was reasonable to conclude that spontaneous combustion must have taken place in the cask. And as there were many more casks of the same material on board, it was considered absolutely necessary for the safety of the ship and cargo, as well as the lives of the crew and passengers, to throw the whole overboard.

"Employed during the rest of the day in hoisting up and throwing overboard the remaining casks of lamp-black, sixty-one in number.

"N.B. Two other casks of lamp-black were observed to smoke, while floating past the ship."—*Ind. Gaz.* March 20.

ON OIL IN HUMAN BLOOD, BY DR. ADAM.

The following brief notice may prove interesting, as it relates to a peculiarity in the human subject, which I have not hitherto met with; nor do I remember to have read of a similar occurrence in medical writings. The body of Serjeant Macdonald was sent from the garrison to the general hospital, for inspection; as certain circumstances had created a suspicion regarding the manner of his death. He had gone to bed in the

the barrack-room apparently in good health, and was found in the morning lying dead on his couch. He had had a quarrel, it was stated, the preceding evening, with some of his comrades, and, it was currently surmised, had met with his death by violence through their means. Under this impression, the body was directed to be examined with great care, and a report made of the appearances on dissection.

The subject was rather corpulent, and, from incipient putrefaction, much swelling and discoloration existed about the head and neck. On removing the scull-cap, some blood, which escaped from a sinus wounded in the dissection, was observed to present a singular oily appearance on its surface. When minutely examined, this was found to proceed from an oil swimming about in the fluid, in the form of small globules. In consistence it resembled olive-oil; but in colour approached more to that of amber, or of hot drawn castor oil. In the substance of the brain, slight indications of congestion presented themselves, but no decided inflammatory appearance. The abdomen was opened, and the blood in the cava ascendens found to contain the same oily matter in great abundance, as was also the case with the femoral and other vessels of the lower extremity; and it evidently pervaded the whole venous system. In proportion to the mass of blood, it existed in considerable quantity, and might be collected by means of a spoon, with great ease. A quantity of the oil thus procured, with some adherent blood, was set aside for analysis; but putrefaction speedily taking place, prevented the examination. No visible disease existed in any of the viscera, whether of the thorax or abdomen. It was afterwards ascertained that this man had been intoxicated the night previous to his decease; but he was in general of sober habits, and enjoyed a perfectly sound and healthy frame.*—*Trans. Med. Soc. Calcutt.*

* Since the above notice was presented to the Calcutta Medical and Physical Society, I have observed in the Edinburgh Philosophical and Medical Journals, that a similar oil is described as having been found in the blood of the living subject, by Dr. S. Trail of Liverpool. The oil in these instances was combined with the serum in the form of an emulsion; and it is not improbable, that during life the same union existed in the case now detailed. Putrefaction, however, having commenced before the body was opened, we had no opportunity of witnessing the natural appearance of the fluid, or of ascertaining the relations which its elementary parts may have borne to each other. Judging from its appearance, I should say it was much more abundant than the proportion stated by Dr.Trail in his cases. The blood, too, was of a thicker consistence, and considerably darker in colour than usual; and the oil which was swimming on the surface, as stated above, could with ease be separated from the general mass. It may be worthy of remark, that on the evening this notice was made to the Society, a member then present, Mr. Veterinary-Surgeon Hodgson, stated, that he had more than once observed a similar oil in the blood of the horse; but although his attention was particularly attracted to the circumstance at the time, he was totally at a loss to account for it.—T. A.

REPETITION OF THE COMPARISON OF THE RATE OF THE MERCURIAL AND SPIRIT THERMOMETER.

Dr. Wildt of Hanover has made a new set of experiments for ascertaining the real indications of the spirit thermometer, chiefly with a view to the employment of that instrument in the Register Thermometer of Rutherford. His results, which are stated below, do not differ materially from those of Deluc. The observations are made at intervals of five degrees of Reaumur's scale.

Mercury.	Spirit.	Mercury.	Spirit.
−45	−28·50	20	16·48
40	25·92	25	20·97
35	23·19	30	25·60
30	20·32	35	30·38
25	17·30	40	35·31
20	14·13	45	40·38
15	10·82	50	45·60
10	7·36	55	50·97
5	3·75	60	56·48
0	0·00	65	62·14
+5	3·90	70	67·95
10	7·95	75	73·90
15	12·14	80	80·00

(*Kastner's Archiv für die Gesammte Naturlehre*, Dec. 1825.)

TANKERVILLE COLLECTION OF SHELLS.

The only opportunity of becoming possessed of any part of the splendid collection of shells, formed by the late Earl of Tankerville, is about to be offered to the public. This matchless Collection, together with a large addition from several other collections, consisting in the whole of about four thousand species, and more than forty thousand specimens, will soon, we understand, be brought forward for sale, by public auction, by Mr. G. B. Sowerby. Mr. S. has circulated a plan for the disposal of this collection, affording peculiar advantages to scientific collectors, copies of which may be obtained at 156 Regent Street, where the collections are exhibiting.

NAVIGATION OF THE RHINE.

From a paper in the *Allgemeine Zeitung* and other sources, we have collected a few facts respecting the navigation of this celebrated river—a subject on which our common geographical books gives us no information. The Rhine rises in Switzerland, and after a course of 700 miles falls into the sea in Holland, passing through, or touching, the territories of Baden, Bavaria, Darmstadt, Nassau, and Prussia. The basin of the river,

river, or the country over which its branches extend, includes an area of 70,000 English miles, and is inhabited by 14,000,000 of persons. The navigation extends without interruption to Schaffhausen, 500 miles from the sea, but above Manheim it is much obstructed by islands and shoals. From the sea to Cologne, a distance of 160 miles, there are, according to the German writer, ten or twelve feet of water; and he adds, that the river, deriving its water chiefly from the melting of Alpine snows, is deeper in summer than in winter. Cogan informs us, that from Cologne to Mentz, a distance of one hundred miles, the river is navigated by shallow vessels of one hundred or one hundred and fifty feet long, by 30 or 40 feet in breadth, and drawing about five feet water, which are sometimes tracked and sometimes impelled by sails. From Mentz up to Basle in Switzerland, according to information we received two years ago (from a gentleman who had made inquiries on the subject, with a view to the introduction of steam navigation), nearly the same depth might be obtained; but the numerous shoals, islands, and rocks, render the channel intricate. Were a short canal made at Schaffhausen (where the fall is only 50 feet high), the line of inland navigation for small sailing vessels might be extended to the head of the Lake of Constance, and the produce of the Alpine valleys of Switzerland and Bavaria might be conveyed by water to Holland or England. Its larger branches too, the Maes, the Moselle, the Mayne, the Neckar, &c. are generally navigable to some distance from the mouths. Were such a magnificent natural canal placed in the midst of fourteen millions of Englishmen or Americans, it would be the theatre of the most multifarious and animated internal commerce on the face of the globe. But the people want enterprise, capital, and a commercial spirit; and, what is still worse, they are parcelled out among half a score of different princes, who harass the trade of each other's subjects by imposts and retaliatory restrictions; and who all unite in oppressing the foreign trader, by heavy exactions. "Nothing," says Riesbeck, "displays the constitution of the German empire in a better light than the navigation of the Rhine. Every prince, so far as his domain on the banks extends, considers the ships that pass as the vessels of foreigners, and loads them without distinction, with almost intolerable taxes. In the 12th and 13th centuries, the princes of the Rhine compelled the emperors to give them so many customs as to make every city a custom-house; originally all the customs belonged to the emperors, but the want of men, money, and other services, obliged them to part with most of them to purchase friends. While the anarchy lasted, every one took by force what was not given him by free will,

 and

and at the peace they found means to keep possession of what they had stolen. In the small district between Mentz and Coblentz, which, with the windings of the river, hardly makes 27 (German) miles, you don't pay less than nine tolls. Between Holland and Coblentz, there are at least sixteen. Every one of these seldom produces less than 25,000 or 30,000 guilders a year." The tolls were not levied for supporting beacons and removing shoals, or for any other purpose useful to commerce; but were, in general, a sort of black-mail, or premium paid by the merchant to escape being plundered. What was robbery at first,—time, which consecrates so many abuses, has improved into vested rights; and of course, if the trade of the river is to be released from these burdens, the fee-simple must be bought up by the State, or compounded for in some other way. So much is trade shackled by these vexatious imposts, that the exports of fourteen millions of people by the river, amounts only, according to the German editor, to six millions of guilders (600,000*l.*), and the imports to forty millions (4,000,000*l.*). This is marvellously little, when we recollect that the inhabitants of Rhenish Germany are among the most industrious and civilised in continental Europe, even though we allow (what is not probable) that half of the foreign trade of the country is carried on through other channels. The exports and imports of Pennsylvania alone, with one million of inhabitants, exceed this sum. As for the internal trade, which is equally burdened with the foreign, the German editor gives no estimate of its amount; but from the scattered notices to be found in travellers, we know that it has long been in a languid and depressed state. The Hudson, flowing through a country inhabited by less than two millions of people, was navigated by 2,000 sloops some years ago. We question if the Rhine has nearly as many at this day. And no less than 78 steam-boats plied on the Mississippi, at a time (1823) when a single vessel of the kind had never been seen on the Rhine.—*Scotsman.*

LIST OF NEW PATENTS.

To Erskine Hazard, a citizen of the United States of North America, but now residing in Norfolk-street, Strand, engineer, for an invention, communicated from abroad, with additions, of a method for preparing explosive mixtures and employing them as a moving power for machinery.—Dated the 12th of August, 1826.—2 months allowed to enrol specification.

To John Thomas Thompson, of Long Acre, camp equipage maker, for improvements in making metallic tubes, whereby strength and lightness are obtained, and for applying them, with

with various other improvements, to the constructing of the metallic tube and other bedsteads.—17th of August.—2 mons.

To John Charles Schwieso, of Regent-street, musical instrument-maker, for improvements on stringed musical instruments.—22d of August.—6 months.

To Timothy Burstall, of Leith, and John Hill, of Bath, engineers, for improvements in the machinery for propelling locomotive carriages.—22d of August.—6 months.

To James Yandall, of Cross-street, St. John's, Surry, for improvements on apparatus for cooling and heating fluids.—24th of August.—6 months.

To Francis Halliday, of Ham, Surry, esquire, for improvements in raising or forcing water.—25th of August.—6 mons.

To William Downe, senior, of Exeter, plumber and brass-founder, for improvements on water-closets.—25th of August.—6 months.

To Robert Busk, and William King Wesly, of Leeds, Yorkshire, flax-spinners, for improvements in machinery for heckling or dressing, and for breaking, scutching, or cleaning hemp, flax, or other fibrous substances.—29th of August.—6 months.

To William Day, of the Strand, for improvements on bedsteads, which improvements are also applicable to other purposes.—31st of August.—6 months.

To Thomas Robinson Williams, of Norfolk-street, Strand, for a machine for separating burs or other substances from wool, hair, or fur.—18th of September.—2 months.

To Thomas Robinson Williams, of Norfolk-street, Strand, for an improved method of manufacturing hats and caps with the assistance of machinery.—18th of September.—6 months.

To John Riste, of Chard, lace-manufacturer, for improvements in machinery for making bobbin or twist net.—4th of October.—2 months.

To Francis Halliday, of Ham, Surry, equire, for improvements on apparatus used in drawing boots on and off.—4th of October.—6 months.

To Theodore Jones, of Coleman-street, London, for an improvement on wheels for carriages.—11th of October.—6 mo.

To William Mills, of Hazelhouse, Bisley, Gloucestershire, for an improvement in fire-arms.—18th of October.—6 mon.

To William Church, of Birmingham, for improvements in printing.—18th of October.—6 months.

To Samuel Pratt, of New Bond-street, for improvements (partly communicated from abroad) on beds, bedsteads, couches, seats, and other articles of furniture.—18th of October.—6 months.

To William Busk, of Broad-street, London, esquire, for improvements in propelling boats and ships or other vessels or floating bodies.—18th of October.—6 months.

To James Viney, of Shanklen, in the Isle of Wight, colonel of artillery, and George Pocock, of Bristol, gentleman, for improvements in the construction of cars or other carriages, and the application of a power hitherto unused for that purpose to draw the same, which power is also applicable to the drawing of ships and other vessels, and for raising weights and for other useful purposes.—18th of October.—6 months.

Meteorological Results from Observations made at Sandwich, in Kent, July 17, 1826. *By* W. H. Weekes, *Esq.*,* *Surgeon.*

Lat. 51° 7′ N. Long. 1° 18′ E.; seven feet above the level of the sea.

Time of Observation.	Mean of three Thermometers.	Barometer.	Coventry's Hygrometer.	General Remarks.
A.M. 6	57°	29·63	41	Clear blue sky; breeze N.W. by W.
7	60	29·65	43	Few detached cirri.
8	62	29·70	44	Breeze N. by W.
9	65	29·72	42	Cumulo-floccosus.
10	68	29·76	42	Clouds increasing; breeze E.
11	70	29·77	35	Clear & clouds; gentle breeze N. by W.
Merid.	73	29·86	29	Ditto ditto ditto
P.M. 1	72	29·87	15	Ditto ditto ditto
2	74	29·84	9	Ditto ditto ditto
3	71	29·00	7	Ditto ditto ditto
4	71½	29·00	6	Less cloudy
5	71	29·00	5	Ditto
6	71	29·00	5	Clear sky
7	69	29·00	17	Ditto
8	63	29·00	38	Cirri reappear
8^h 58^m	61	29·00	41	Sun-set; moon risen unclouded.

Note.—All the philosophical instruments were placed in the same situation out-of-doors, screened only from the direct rays of solar heat and light, and five feet from the surface of the earth. The effects of radiation were completely avoided, and the thermometers suspended from rods of glass, by means of silk threads, three feet in length.

* These observations were made in consequence of the wish for contemporaneous meteorological observations, lately expressed by the Royal Society of Edinburgh, which has also been complied with by Mr. Bevan: see the present vol. p. 152.

Register of the Thermometer at Wick in the County of Caithness, near the Extremity of Scotland, at Half-past seven in the Morning, and Half-past eight in the Evening, for the Year 1825.

Days.	January.		February.		March.		April.		May.		June.	
	Morn.	Even.	Morn.	Even.	Morn.	Even.	Morn.	Even.	Morn.	Even.	Morn.	Even.
1	37	41	36	31·33	36	40	41	42	46	43	52·50	52·67
2	36	39	32·33	40·50	40	37	42·33	47	44·50	44	54	54.50
3	34·50	36	28	27	34	37	50	48	43·50	45	46	46·50
4	32	33·50	25	29	34	37·67	45	48·50	48	46	50	46·50
5	31	30·40	29	36.50	37	39·67	45·50	50·50	46	48	46	46·50
6	35	39	34	33·50	38·50	41·33	49	44	47	54·50	47	50
7	43	39	36	37	40	41	49	54	49·50	51	52	54
8	44	43	35·50	34	41	46	49·50	48·67	51·50	50·50	53	48·50
9	42	42.67	39	43	42·50	45	43	43·67	50	49·50	53·50	51·50
10	44	46	44·50	42·50	41·50	43·67	45·50	49·50	50	49	50	59
11	46·50	46.33	45·50	47·50	41·50	39	47	36	50	47	61	64·50
12	42·50	43·50	45·50	43·33	34·50	40·50	32	31·50	49	46	54	54·50
13	42	47	48	49·50	39·50	40	31	33	46	48	55·50	55
14	44	43	42·50	41·50	38	36·67	40	48·33	50	48·50	56·50	60
15	44	43·50	41	42	36	37·67	50·33	52	48·50	46	57	59
16	41	37	39	38·33	36·67	38	43	37	50	49·50	56·50	51
17	33	40	41	39	37	38	39	39	50	50	52	51
18	43	39·50	39·50	40·67	40	42	39	33·50	50	49	50	50·50
19	39	38·50	35·50	43·50	44·33	41·33	36	42·50	50	49·50	51·50	49
20	38·50	41	38	45·50	39	45·33	42·33	46	49	49·50	47	46·50
21	37	37	43	41	48	47	46	47	53·67	48·67	47	46
22	34	38	38	41·33	46	47	45	41	50	49	48	52
23	37·50	36	42·67	42·33	44	44·33	52	37	47	45·33	56	53
24	35	36	40·50	41·67	42	43	39	40·33	45·33	42·67	55	53
25	31	31·50	41·50	40·50	39	40	38	43	45	48	54	53·50
26	38	44	36	38	41	45·50	44	44	47	44	55	59
27	47	38	39·50	34	47	46·33	42	44	42·67	39·50	56	60
28	34	38·50	33·50	38·50	42	44	44	45	38	39	57	58
29	43	45·50			45	44	44	46·33	40	42	56·50	56
30	47	43			44	42	45	43·50	42	46·67	57	56
31	39	41·50			44	41			46·50	52·67		

Days.	July.		August.		September.		October.		November.		December.	
	Morn.	Even.	Morn.	Even.	Morn.	Even.	Morn.	Even.	Morn.	Even.	Morn.	Even.
1	55	50	60·33	63	60	60	54	55	44	38·50	39	37
2	56	57	63	59	57	55	54·50	56	35·33	37·50	40·50	36·67
3	60·50	54	62	56	55	51	56	56·50	38	36	37·67	35·50
4	57	53	53	55	52	48	57·33	53	41·50	35.50	38	35
5	56	54	53	52	50	54·33	56·67	57·67	34	43	32	37
6	54·50	55	56	50	55	53	53·50	52·50	36·33	33·33	40	42
7	56·50	54	50	50	56	54·50	48	49	34·50	37	41·50	40·50
8	57	54	52	55	55	53	50·50	55	33·50	32	44	45·50
9	54	52·50	54	54·50	52	53	55·50	54·33	32·50	33·67	43	42·50
10	55	54	56	54	55	57	52	48	29	32	44·67	43
11	53·50	53·67	53	50	57	56·33	48·50	50·33	33·67	34·50	43·50	42
12	52·50	57	54	54	56·67	55·67	54	53	34·50	37	45	45·67
13	58	64	57·50	58	57·33	54	50·33	54·33	40·50	38·33	44·33	33
14	61	63	55	50	55	52·50	55	50	40	36·50	37	40·50
15	61	60	55	53	51·50	56	46·50	48·50	31	38·33	36·50	39·67
16	65	60	54	53	56·33	56·50	50	45	39·50	40	40·50	40
17	67	66	54	53	54	54	44	40·33	41	40	41	46
18	62·50	66	53	54	57	56·33	41·33	38·50	42·50	48	45·50	46·67
19	57	59	56	59	56	55·50	37	37·50	40·50	39·50	45	46
20	59·50	57	61	58	54	55	35·67	36·33	59	48	45·33	47
21	58	54	60	59	54	53	40·50	41·67	40·67	42	45	47
22	53	54	67	48	50	51	39·67	43·50	38·50	40	47	31
23	53·50	53	51	52	43	49	40·50	50	37	41·50	39·50	40
24	54	53	50	49	53	52	45	35·67	40·67	36	38·67	40·67
25	55	52·50	54	52	54·33	52·50	41·50	42	34	39.50	37	34
26	55	53	51·50	51	52	51·50	43	38	43·50	36	32	31
27	57	53·50	54	54	51	52	42·50	38·50	30	31·50	32	35·50
28	56	54	55	58	65·50	53	50·50	56	32·50	40	37	36
29	55	63	57	59	53	55	43·50	46	38	32·50	30	28
30	66	62	57	66	55	56	42·50	44	37	38	35	26
31	61	61	65	67			41	40·50			30	32

Results of a Meteorological Journal for September 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.—From a review of the weather this month, we find that it has been alternately wet and dry, it having rained on 15 days, on some of which heavy showers fell here; and the depth at the ground is half an inch more than fell in the three preceding months. The rain was often accompanied with early equinoctial gales, but in general it was so warm as to be very beneficial to vegetation, and to the rapid growth of grass; insomuch that the grass-lands in this neighbourhood are now clad with verdure equal in appearance and quantity to that in the middle of a warm spring. Early in the mornings of the 11th, 12th, and 13th, there were slight hoar frosts; the temperature of the external air, however, has been pretty uniform throughout the period, and the mean is $1\frac{1}{6}$ degree higher than the mean of September for the last 10 years.

The temperature of spring water is nearly at its *maximum* height, and, as usual at this time of the year, almost at a stand, but warmer than it has been since the autumnal equinox in 1822; consequently the ground is warmer than it has been since that year.

The last dry and hot summer has produced greater crops of grapes throughout the country, than has been seen for many years past: the best white and black grapes have been sold in our market at two-pence per pound; and good apples at six-pence per gallon.

The swallows have congregated here since the middle of the month, and the last bevy migrated on the 27th instant, for a more genial climate, making their stay this year 23 weeks. As an instance of the remarkable instinct of swallows to change their situations at the beginning of autumn, we shall state the days on which they have migrated from this neighbourhood for the last eight years, namely, in

1818, Sept. 29th.	1821, Oct. 8th.	1824, Sept. 24th.
1819, Oct. 1st.	1822, Sept. 19th.	1825, Oct. 11th.
1820, Oct. 10th.	1823, Sept. 25th.	1826, Sept. 27th.

The greatest deviation in the time of their departure for this period is only three weeks, which may be considered as regular as the *falls* in those years. Much *gossamer*, the substance of spiders' webs, has prevailed this month in the gardens, fields, and about the houses.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are one lunar and two solar halos, eight meteors, three rainbows, lightning in the evening of the 24th; and eight gales of wind, or days on which they have prevailed; namely, three from the N., three from N.E., and two from the N.W.

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·24,	Sept. 15th—Wind N.E.
	Minimum	29·20,	Ditto 7th—Wind N.W.
Range of the mercury . .		1·04.	

	Inches.
Mean barometrical pressure for the month	29·890
——— for the lunar period ending the 2nd inst. .	29·966
——— for 15 days, with the Moon in North declin.	29·847
——— for 14 days, with the Moon in South declin.	30·085
Spaces described by the rising and falling of the mercury	5·440
Greatest variation in 24 hours	0·670
Number of changes	18·

Thermometer	Maximum	74°,	Sept. 4th—Wind S.W.
	Minimum	48	in several nights.
Range		26	
Mean temp. of the external air		61·33	
——— for 31 days with the Sun in Virgo		62·10	
Greatest variation in 24 hours		20·00	
Mean temp. of spring water at 8 o'clock A.M. . . .		55·09	

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air .	100 in the evening of the 8th.
Greatest dryness of ditto . . .	43 in the aftern. of the 11th.
Range of the index	57
Mean at 2 o'clock P.M. . . .	58·6
—— at 8 o'clock A.M. . . .	70·0
—— at 8 o'clock P.M. . . .	73·9
—— of three observations each day at 8, 2, and 8 o'clock	67·5

Evaporation for the month	2·70 inch.
Rain in the pluviameter near the ground .	4·555
Rain in ditto 23 feet high	4·220

Prevailing wind, S.W.

Summary of the Weather.

A clear sky, 5; fine, with various modifications of clouds, 12½; an overcast sky without rain, 6½; foggy, ½; rain, 5½.—Total 30 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
20	10	26	0	24	18	17

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
2	5	3	4	3	6½	2½	4	30

A METEORO-

A METEOROLOGICAL TABLE: *comprising the Observations of Dr.* BURNEY *at Gosport, Mr.* J. CARY *in London, and Mr.* VEALL *at Boston.*

Gosport, at half-past Eight o'Clock, A.M.						Clouds.							Evaporation.	Rain near the ground.	Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.			Lond. 1 P.M.	Bost. 8½ A.M.	London. 8 A.M.	London. Noon.	London. 11 P.M.	Boston 8½ A.M.	London.	Boston.	London.	Boston.	Wind.
Sept. 1	29·77	62	54·60	72	E.	1	...	1	...	1	1	1	...	0·370	29·80	29·33	61	68	60	62·5	...	...	Cloudy	Fine	W.
● 2	29·70	58	......	94	N.	...	...	...	...	1	1	1	...	·400	29·70	29·35	60	61	59	63	...	...	Rain	Cloudy	SE.
3	29·74	63	......	72	N.	1	...	1	...	1	1	1	0·30	·010	29·82	29·37	60	65	59	62	...	...	Showers	Cloudy	NE
4	29·90	67	......	70	S.	1	...	1	...	1	...	...	...	......	29·94	29·42	65	70	60	60	...	...	Fair	Cloudy, rain *p.m.*	calm
5	29·94	64	......	60	NW.	...	...	1	...	...	1	1	...	·370	29·93	29·47	60	60	55	60	...	·01	Rain	Cloudy	NW.
6	29·49	64	......	83	S.	...	...	1	...	1	1	1	·15	·320	29·30	29·13	51	62	50	57·5	...	...	Rain	Do. rain *a.m.* & *p.m.*	NW.
7	29·20	54	55·00	75	NW.	...	...	1	...	1	1	1	...	·270	29·40	28·80	50	55	51	55	...	·69	Do. h^h^ w^d^	Rain and stormy	NW.
☽ 8	29·63	62	......	82	SW.	1	1	1	...	1	...	1	...	·290	29·60	29·20	54	65	52	53·5	...	·18	Showers	Rain	NW.
9	29·72	58	......	75	N.	1	1	1	...	1	1	1	·15	......	29·80	29·36	54	58	53	55	...	·06	Cloudy	Cloudy, rain *p.m.*	NW.
10	30·04	58	......	60	W.	1	1	1	...	1	1	1	...	......	30·00	29·54	55	62	51	56	...	·03	Fair	Fine	W.
11	30·20	55	......	58	NW.	1	1	1	...	1	...	...	...	......	30·20	29·75	52	61	51	56·5	...	...	Fair	Fine	NW.
12	30·22	56	......	60	NW.	1	1	1	...	1	...	...	·30	......	30·20	29·77	53	64	52	58	...	...	Fair	Fine	NW.
13	30·09	57	55·10	63	NW.	1	...	1	...	1	...	...	...	......	30·05	29·60	52	65	50	57	...	...	Fair	Fine	W.
14	29·93	60	......	62	SW.	...	...	1	...	1	1	1	...	·095	29·93	29·40	58	68	51	59	...	...	Cld^y^ rain	Cloudy	W.
15	30·15	55	......	56	NE.	1	...	1	...	1	1	...	·25	......	30·23	29·87	50	61	50	51	...	...	Fine	Fine	N.
○ 16	30·23	57	......	56	E.	1	...	1	...	...	...	...	...	......	30·26	29·92	55	63	52	58	...	...	Fine	Fine	calm
17	30·00	61	......	70	E.	1	1	1	...	1	...	...	...	......	29·97	29·62	57	70	55	59	...	...	Fine	Fine	calm
18	29·77	65	......	65	NE.	...	...	1	...	...	1	1	·35	·740	29·80	29·50	60	59	56	61·5	...	...	Rain	Cloudy, rain *p.m.*	SE.
19	29·86	61	......	76	SW.	1	...	1	...	1	...	...	...	......	29·90	29·35	59	67	55	64	...	1·43	Fair	Cloudy	calm
20	29·79	60	55·10	85	NE.	...	...	1	...	...	1	1	...	·020	29·83	29·44	57	64	51	60	...	·04	Cloudy	Cloudy, rain *a.m.*	E.
21	29·98	55	......	61	NE.	...	...	...	...	1	...	...	·40	......	30·05	29·65	55	61	48	58	...	...	Fair	Cloudy	E.
22	30·04	54	......	58	NE.	...	...	...	...	...	...	...	...	......	30·12	29·73	49	59	45	57	...	...	Fine	Cloudy	E.
23	30·01	56	......	60	E.	1	...	1	...	1	...	1	...	·085	30·03	29·65	49	60	55	58	...	...	Do. r^n^ n^t^	Fine	E.
☾ 24	29·70	63	......	78	SE.	...	...	...	...	1	1	1	·35	·820	29·80	29·37	57	65	60	57	...	...	Rain	Cloudy, rain *p.m.*	E.
25	29·62	66	......	69	S.	1	1	1	...	1	1	1	...	·400	29·70	29·16	59	61	59	62	...	·21	Rain	Cloudy, rain *a.m.*	E.
26	29·79	62	55·25	89	SW.	1	1	1	...	1	1	1	...	·245	29·85	29·33	60	66	60	60·5	...	·10	Rain	Cloudy	W.
27	29·94	60	......	71	SW.	1	...	1	...	1	1	...	·25	......	29·98	29·27	59	65	60	60	...	1·35	Fair	Fine, rain *a.m.*	W.
28	30·15	63	......	68	SW.	1	1	1	...	1	1	...	...	......	30·14	29·57	61	66	56	61	...	...	Fine	Fine, rain *p.m.*	W.
29	30·12	64	......	75	SE.	1	...	1	...	1	...	...	...	......	30·12	29·63	57	67	60	58	...	·08	Cloudy	Fine	W.
30	29·77	67	55·50	76	SW.	1	1	1	...	...	1	1	·20	·120	29·80	29·20	66	67	55	66	...	...	Showers	Fine	W.
Aver. :	29·883	60·23	55·09	70·0		20	10	26	—	24	18	17	2·70	4·555	29·91	29·45	56	63	54	58·8	2·20	4·18			

THE

PHILOSOPHICAL MAGAZINE
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30th *NOVEMBER* 1826.

XLVII. *On the Grounds for adopting the Ellipticity of the Earth deduced by Captain* Sabine *from his Experiments with the Pendulum in his Work lately published. By* James Ivory, *Esq. M.A. F.R.S.**

THE experiments with the pendulum are now very numerous; and as different ellipticities may be deduced from different combinations of them, it becomes a question, How we are to distinguish the true ellipticity, from others that may be occasioned merely by accidental irregularities. We do not here allude to small changes in the ellipticity, of the same order as the unavoidable errors of observation, which must occur in different combinations of the most accurate experiments; but to such considerable variations as may lead to different opinions about the figure of the earth.

Suppose we have 25 independent experiments, that being the number we owe to the labours of M. Biot, Captain Kater, and Captain Sabine. By applying the method of the least squares, an ellipticity is obtained which, we will allow, represents all the 25 experiments better than any other. But the experiments employed in the calculation are much more numerous than is necessary for determining the figure of the earth. If the earth be an elliptical spheroid two experiments alone, one near the equator and the other at a distance from it, are sufficient for finding the ellipticity. There is no doubt an advantage in combining a great number, provided they are consistent among themselves; because by this means the unavoidable discrepancies of observation are in some degree compensated, and a mean result is obtained that must approach very nearly to the true figure. But when many experimental quantities are combined in one calculation, it is difficult to distinguish those which are consistent with one another, and really belong to the true figure of the earth, from others which, on account of accidental anomalies, cannot possibly be reconciled with the same figure, at least without admitting very considerable errors. It may be said indeed, that

* Communicated by the Author.

upon applying the general formula for the length of the pendulum which has been deduced from the whole of the experiments, to every individual case, the discrepancies of calculation will show the consistency of the experiments, and will enable us to judge of the degree of accuracy with which they are represented by one figure. But on the other hand, it may be alleged that the mean figure deduced from a great number of experiments may be considerably different from the true figure belonging to the consistent observations alone; and that the discrepancies of calculation are therefore not an unexceptionable means of forming a just notion of the distribution of gravity on the earth's surface, and of the anomalies to which it is subject. In one case only there is no doubt that the mean figure coincides with the true one; and that is, when all the errors of calculation are small, and of the same order with the errors of observation. But when the errors are more considerable, the mean ellipticity deduced from a great number of experiments, cannot be adopted, without further investigation, as a safe determination of the figure of the earth. In order to ascertain this point on sure principles, it seems necessary to subdivide the whole of the experiments into partial combinations; to investigate the ellipticity of every separate combination; and finally, to examine whether all the results agree or disagree. If they agree with one another and with the ellipticity obtained from the whole of the experiments, we are then certain that the figure of the earth has been rightly determined. But if the results are discordant, the proper inference is, not that the earth has any particular ellipticity, but that the observed quantities are inconsistent with one another; and no exact knowledge with respect to the distribution of gravity on the earth's surface, can be obtained, but by separating the experiments that are consistent and belong to the mean figure of the earth, from those that are anomalous.

In 32 independent experiments with the pendulum, made by different observers with the most improved apparatus and after the methods of experimenting have been fully perfected, I showed, in the last Number of this Journal, that 26 are consistent among themselves, and concur in giving the same figure to the earth. The resulting ellipticity is about $\frac{1}{303}$ which agrees with what is deduced from the inequalities of the lunar motions. If we apply a proper mathematical method to any sufficient combination of the experiments in my Table, the same results will always be obtained, or, at least, the small differences will be of the same order as the unavoidable errors of observation. But the remaining six experiments do not belong to the same elliptical figure with the others, at least,

without

without admitting great errors. This discordance is not to be accounted for by the particular magnitude assigned to the ellipticity. It is a real inconsistency that cannot be removed by any probable change in that element. To prove this we need only compare the lengths of the pendulums at the six stations, with the lengths determined on the neighbouring parallels of latitude.

Having now laid down some principles by which our researches are to be guided, we proceed to consider the grounds for adopting the ellipticity assigned to the earth by Captain Sabine in his late work. It must be observed that we do not here discuss the merits of the observer, or the accuracy of his operations. Our reasoning is founded upon the experiments as they have been laid before the public. While the facts remain the same, we do not conceive that our arguments can be controverted.

The conclusions of Captain Sabine will be found to rest ultimately upon his experiments near the equator. Seven of his stations are within less than 20° of the equatorial circle, viz.

Station.	Latitude.	Pendulum.
Bahia	12° 59′ 21″ S.	39·02425
Ascension	7 55 48	39·02410
Maranham . . .	2 31 43	39·01214
St. Thomas . . .	0 24 41 N.	39·02074
Sierra Leone . .	8 29 28	39·01997
Trinidad	10 38 56	39·01884
Jamaica	17 56 7	39·03510

The extreme irregularity of these experiments is immediately apparent. The pendulum at Trinidad, 10°½ from the equator, is very considerably less than the equatorial pendulum at St. Thomas. But in order to judge more distinctly of the discrepancies, it will be proper to deduce the equatorial pendulum belonging to every individual experiment. We have to find L from the formula,

$$l = \mathrm{L} + f \sin^2 \lambda :$$

and, although f is not exactly known, yet its value cannot be less than 0·2, nor greater than 0·21: on these two suppositions we obtain as follows:

Equatorial Pendulum.

	$f = 0·2$	$f = 0·21$
Bahia	39·01415	39·01364
Ascension	39·02030	39·02010
Maranham . . .	39·01175	39·01174
St. Thomas . . .	39·02073	39·02073
Sierra Leone . .	39·01569	39·01539
Trinidad	39·01201	39·01166
Jamaica	39·01614	39·01519

Now

Now the numbers in this Table should be nearly equal, if the seven experiments were consistent, and belonged to the same elliptical figure. This, however, is so far from being the case, that we may discern three different systems in the lengths it contains. One derived from the experiments at Ascension and St. Thomas, having an equatorial pendulum about 39·02; another with an equatorial pendulum about 39·015, comprehending Bahia, Sierra Leone, and Jamaica; and a third comprehending Maranham and Trinidad, with an equatorial nearly equal to 39·0117. If we adopt the last determination and combine it with the experiments at the stations of the Trigonometrical Survey in Great Britain, we shall obtain an ellipticity very little different from that generally received; if we prefer the second and combine it in the same manner, the resulting ellipticity will be very nearly the same with what Captain Sabine has deduced from his calculations; and lastly, if we combine the first equatorial pendulum, namely 39·02, with the same experiments, we shall get an ellipticity still greater than that assigned by Captain Sabine. But it may be said that we ought to adopt the mean equatorial pendulum, resulting from all the seven experiments, in preference to any of the three particular systems above-mentioned. Now this will be found to make the pendulum at the equator about 39·0156, which is hardly different from the second of the foregoing lengths, and almost coincides with the result which Captain Sabine has uniformly deduced from all the combinations of his experiments.

The observations that have been made unvail the whole mystery of Captain Sabine's investigations. They disclose the real reason of that uniformity and consistency of result which are preserved in calculations apparently greatly varied, and which constitute the grand argument in support of the proposed ellipticity. It must be observed, that Captain Sabine has employed no tropical experiments except his own, and that these are a constituent part in all his calculations. Now it requires very little sagacity to discover that the results he obtains can be very little different from what they would be, if the mean of the equatorial pendulums was immediately combined with the northern experiments. It happens that, if we except Drontheim, the experiments at the northern stations are tolerably consistent; and hence it follows that nearly the same ellipticity is brought out in every calculation which includes all the tropical pendulums. Although, therefore, there is a multiplicity of arithmetical operations, there is no accumulative evidence in favour of the result. The same ellipticity is always found, because it may be affirmed that a principal

cipal *datum* on which the calculation turns, namely, the mean equatorial pendulum of the tropical stations, is, in every case, the same.

Although the observations we have made are sufficiently obvious, yet we may add in confirmation of them, that if any of the pendulums near the equator be left out, so as to alter the mean of the equatorial pendulums, the ellipticity will also be changed. Thus Captain Sabine, by a calculation in which all his 13 stations are included, finds the equatorial pendulum equal to 39·01568, and the ellipticity, to ·00346. But if we leave out the stations at St. Thomas and Ascension, the 11 remaining stations will give 39·01374 and ·00340, for the like quantities. Further, if we leave out St. Thomas, Ascension, Sierra Leone, Bahia and Jamaica, the equatorial pendulum will be 39·01213 and the ellipticity ·00336, approaching very nearly to the values usually received. And if we add to Captain Sabine's 13 stations the experiments made by other observers near the equator, for instance, at Madras and Rio Janeiro, the effect produced will be the same, that is, the equatorial pendulum and the ellipticity will be both lessened.

There is an assertion of Captain Sabine at p. 353 of his work, which it will not be improper to notice. "If each of the tropical stations, which I have visited, be severally combined with each of the stations within 45° of the pole, no one result, amidst all the irregularities of local attraction, will be found to indicate so small a compression as that of previous reception." Now this is strictly correct with respect to five of his tropical stations; but I have shown, in this Journal for August last, p. 96, that if the pendulums at Maranham and Trinidad be combined with Captain Kater's seven experiments, the resulting ellipticity will be ·00329 at a mean, which can hardly be accounted different from ·00327, the quantity adopted by the French philosophers.

It follows from all this discussion that the ellipticity assigned by Captain Sabine is obtained only when all his tropical pendulums, unmixed with any other experiments near the equator, are combined with the northern experiments. Whenever the mean length of the equatorial pendulums is altered, either by leaving out some, or by adding the experiments made by others near the equator, the ellipticity will undergo a change. If the mean equatorial pendulum of the tropical stations be 39·0156, the ellipticity will be ·00346; otherwise, it will have a different value. Now it must be allowed that Captain Sabine's seven tropical pendulums are exceedingly irregular and anomalous; and no confidence can be placed in a mean deduced from so few experiments liable to such objections.

objections. But we have his own authority at p. 359, founded upon a more extensive comparison of facts, that the equatorial pendulum is 39·01, and not 39·0156; and this new length being combined with the northern experiments, will bring out an ellipticity about ·00326, the same with that generally received. As the mean quantity 39·01 is fixed and determinate, the natural conclusion seems to be, that ·00326, which depends upon it, is the ellipticity to be adopted in preference to ·00346, which is derived only from certain particular combinations of the experiments, and varies when they are combined in a different manner.

But the truth is, that Captain Sabine's tropical experiments are inconsistent among themselves, and with all the good experiments of other observers. In the present state of our knowledge, five of them, or at least four, can be considered only as anomalies which do not belong to the mean figure of the earth. The irregularities are very great and apparent on inspection. Maranham and St. Thomas may be both reckoned on the equator, the small differences of latitude hardly affecting the length of the pendulum: now if a pendulum that beats seconds at Maranham be transported to St. Thomas, it must be lengthened $\frac{86}{10000}$ of an inch in order to oscillate in the same time; and if it were carried to the pole, its length must be increased not so much as $\frac{2100}{10000}$ for the same purpose. Thus there is a local irregularity between Maranham and St. Thomas, amounting to $\frac{1}{24}$ of the whole increase of gravity from the equator to the pole. It must be acknowledged that when it shall be indisputably proved that inequalities so great take place in the distribution of gravity, we can hope to gain little in point of accuracy by employing the pendulum for investigating the figure of the earth.

Nov. 6, 1826. J. Ivory.

[A continuation of this Paper, received while the present sheet was in the press, will be given in a subsequent part of this Number.—Edit.]

XLVIII. Decas septima novarum Plantarum Succulentarum; *Autore* A. H. Haworth, *Soc. Linn. Lond.—Soc. Horticult. Lond.—necnon Soc. Cæs. Nat. Cur. Mosc. Socio, &c. &c.*

To the Editor of the Philosophical Magazine and Journal.

Dear Sir,

The seventh *Decade* of my new Succulent Plants being now completed, allow me to request that it may be admitted into an early Number of your interesting Magazine and Journal.

It is composed entirely of new and unrecorded species of *Mesem-*

Mesembryanthemum, and exhausts for the present my stock of novelty in that vast and polymorphous genus; which seems in a fair way of rivalling, at least in number, as I have already observed to you, the more extensively discovered Heaths.

The species of *Mesembryanthemum* here described, are all now flourishing in our inexhaustible source of novelty, the royal gardens of Kew, save *one;* and were all, save that one, (which is in my own collection, and one other from New Holland) collected in the distant regions of South Africa, by our old and skilful friend Mr. Bowie; who transmitted them safely to those rich gardens, and where I have been graciously permitted to describe them.

Hoping the communication may be acceptable to your botanical readers, I remain, yours, &c.

Chelsea, Nov. 1, 1826. A. H. Haworth.

Classis et Ordo. Icosandria Pentagynia.

Genus, Mesembryanthemum *Auctorum.*

Sectio, Ringentia *Nob.* in Philos. Mag. Aug. 1824, &c.

musculinum. M. (small, trailing, Mouse Chop) foliorum capi-
1. tatorum marginibus carinâque subunidentigeris, ramis prostratis subsemipedalibus.

Florebat in regio horto Jul. &c. 1826. G. H. ♃. s. ♄.

Obs. Facies *M. murini*, sed in ramis prostratis mirabiliter differt, necnon in punctis albis majoribus crebrioribus, dentibusque paucioribus foliorum duplò minorum. Fortè magìs affine *M. erminino* Nob. *in Philos. Mag.* nihilominùs ambobus longissimè differt in petalis lineam latis, nec capillaceis. *Flores* sæpè subpedunculati (antemeridiani?) vix unciales omninò inodori. *Calyx* 5-phyllus turbinatus, foliolis æqualibus, margine omni submembranaceo. *Corolla* flava *petalis* supernè purpurascentibus. *Stamina* paucula exigua, basi ad lentem pubescentia, *petalis* multoties breviora lutea, uti *antheræ. Styli* 5 stellatim recurvi, *antheris* longè superantes. *Germen* 5-loculare.

In sectione hâc dignoscitur primo intuitu, ramulis sarmentiformibus.—An ab æstu nimis elongatum? Vix; enim abundè florebat. Pone *M. ermininum* locarem, ante *M. murinum.*

Sectio, Linguiformia, acaulia, foliis linguiformibus, &c. *Nob. in Revis. Succ.* 93.

grandiflorum. M. (great-blistered broad-leaved) foliis prælatè
2. linguiformibus

linguiformibus longis grossis, basi intùs grandipustulatis: petalis latissimis.

Obs. In hâc sectione magnum.

Folia disticha humi adpressa sæpè $3\frac{1}{2}$ uncias longa, 15 lineas lata, præpinguia pallidè viridia. *Flores* subinodori sæpè sessiles maximi, 3—4-unciales, *petalis* luteis duplici serie, et subindè 3—4-lineas latis obtusis. *Capsula* subconica.

β. foliis saturatioribus.

Obs. Species conspicua valdè distincta. Pone *M. fragrans* in propriâ subsectione locarem.

Sectio, Difformia. Subacaulia, caudiceve brevi, foliis obliquè cruciatis difformibus, semicylindricis crassis, &c. *Nob. in Revis. Succ.* 101.

bigibberatum. *M.* (lesser difformed) foliis obliquè subcruciatis
3. semicylindricis pallidè viridibus, apicem variantem versùs sæpè bigibberatis: capsulâ depressâ.

Habitat C. B. S. G. H. ♄. Communicavit amicus Dom. Thom. Edwards, succulentarum plantarum assiduus cultor.

Obs. *M. difforme* maximè simulat, at gracilius, duplò minus, (capsulâ ut in eo,) caudice longiore foliis pallidioribus, apice minùs productis, gibberibus distinctioribus. *M. semicylindricum* quoque simulat, at vix varietas, et duplò majus: inter ea ergo id locarem.

Sectio nova, Procumbentia, caulibus senectis dodrantalibus procumbentibus perennibus, foliis connatis basi, longis semiteretibus cylindricisve, floribus polygynis.

In hâcce novâ sectione *M. tricolor* (Nob. Revis. 111.) et *M. procumbens*, (Nob. in loco) collocarem; poneque ea, sequens; ut infrà.

purpureo-album. *M.* (purple-white, flowered) foliis viridibus,
4. triquetro-semicylindricis punctatissimis, supremis confertis: ramis brevibus angulatis prostratis sulcato-striatis lutescentibus.

Obs. *M. loreo* Linn. parùm affine, at foliis non basi gibbis, flore intùs albo, nec purpureo. Simulat magìs *M. procumbens*, sed ramis brevioribus, foliis confertioribus. *Flores* terminales frequentes, 1—3-nati, *pedunculis* filiformibus cylindraceis æqualibus 3—18-linearibus, (centrali duplò majore,) basin versùs bibracteatis.

Càlyx

Calyx 5-phyllus subæqualis affinium. *Corolla* biuncialis speciosa, *petalis* albis politissimis, costâ externè latissimè purpureâ. *Stamina* filiformia brevia conico-collecta alba, mox rosea; *antheris* nuptatis fuscis, polline roseo. *Styli* 10, altè rubicundi subulati longi erecti, apice parùm patentes, *staminibus* breviores, externè ad lentem ramentacei. Flores nondum apertos purpureos putares; post expansionem albi, cum argenteo splendore.

Sectio, SARMENTOSA *Nob.* caulibus suffrutescentibus decumbentibus seu prostrato - sarmentiformibus sæpè reptantibus: foliis æquilateri - triquetris: margine aspero: floribus subternatis parvis inflato-bracteatis rubicundis: *stylis* quinque.

validum. *M.* (robust, light green) foliis longis pallidè viridibus
5. margine asperiusculo, ramis robustis rigidis decumbentibus floribus subternatis, bractearum carinis integris.

Obs. *M. rigidicaule* Nob. proximum, *ramis* brevioribus, contiguioribus erectioribus, *foliis* pallidioribus internodiis semper brevioribus, *floribus* (in ambobus autumnalibus) paucioribus, minúsque pedunculatis. *Petala* perfecta rosea rubricostata non examinavi.

In *M. rigidicaule* caulis sæpè duplò longior est quàm in *M. valido*, *folia* subindè internodio breviora, saturatè viridia, marginibus carinâve asperioribus, bractearumque carina in lateralibus floribus (deorsùm præcipuè) singulariter lacera, membranacea. *Flores* decussatim biternati-quinative vix parvi, unciales formosi, *bracteis* amplis latis, *petalis* politis roseis costâ utrinque ruberrimâ. *Calyx* ordinarius. *Stamina* calyce breviora atro-purpurea; *antheræ* pollinosæ luteæ. *Styli* 5, erecti subulati lutei antheras æquantes.

Sectio nova, CARNICAULIA, caulibus sæpissimè elongatis debilibus prostratis, reptantibus, juventute carnoso - crassis, foliis subæquilateri - triquetris crassis sæpè mollibus, floribus solitariis terminalibus rubicundis magnis speciosis hexagynis; capsulâ extùs in adolescentiâ pulposâ.

Obs. Huc referenda *M. Rossi*, *æquilaterale*, *virescens*, et *glaucescens* Nob.—necnon *M. chilense*, *Molini* —quoque ad ea proximum posui.

abbreviatum. *M.* (short-jointed, tufting) cæspitosum: caulibus
6. grossis brevibus prostratis confertis, foliis acutè triquetris

tris crassis viridibus, internodiis diminutis multoties longioribus.

Habitat in Australasiâ. G. H. ♃.

Vigebat in regio horto Kewense A.D. 1825.

Obs. Folia sæpè in optimis plantis parùm incurva biuncialia, 3—4-lineas crassa, et aliquantillum compressa, apice acuto s. submucronulato, (aëre aperto) rufo.

Prope *M. glaucescens* locarem propter similia folia, at multoties confertius in omnibus quàm in eo.

Flores non vidi.

Sectio, Humillima *Nob.* ramis ramulisque gracilibus omninò prostratis reptantibus: foliis parvulis tereti-triquetris incurvis clavatisve; sæpè confertis, et subacinaciformiter triquetris.

debile. 7. *M.* (smooth, weak, reptant) læve: ramis filiformibus parùm compressis: foliis ad caulium subreptantium nodos confertis, obtusè acinaciformiter-triquetris glaucescentibus.

Obs. Multicaule subcæspitosum; ramis numerosis flexuosè prostratis, nodis sæpè radiculigeris: junioribus in aëre aperto subteretibus, sive obtusè subcompressis purpureis internodiis folio frequenter longioribus, lateralibus parvis ramulis subindè suprà acutè planatim semiteretibus.

M. reptanti fortè proximum, at distinctissimum læve, nec asperum, foliis longè minùs congestis et angustioribus, ramulisque triplò gracilioribus.

Flores non vidi:—neque in antiquo *M. reptanti!*

Sectio, Uncinata, Fruticuli, foliis connato-vaginantibus triquetris, carinâ sæpiùs plùs minùs uncinato-dentatâ, &c. *Nob. Revis.* 124.

unidens. 8. *M.* (dwarf uncinate) depressum: rigidum: ramis confertis; foliis subacinaciformibus albidis grandipunctatis, carinâ supernè unidentigerâ.

Obs. Species distinctissima, nullo multùm affine: sed sectionem *Uncinatam* fortè approximat, foliis minùs perfoliatis. Suffrutex densè depressus tertio anno vix semipedalis, ramis brevibus rigidis subangulatis. *Folia* per lentem tenuiter cartilagineo-marginata atque cuspidata, apicem versus insuper carinam sæpiùs unidentigera, dente brevi albo cartilagineo rectim extanti. *Flores* non vidi.

Obs. Foliorum vagina supernè lineolâ impressâ s. sulcatâ

sulcatâ sed minùs quàm in *M. semidentato;* cui forsan proximum, discrepante vaginâ.

Sectio, DIGITIFLORA *Nob.* suffrutices erecti, foliis subcylindraceis, ad solem micantibus atomis innumeris, floribus mediocribus albis stenopetalis ternatis solitariisve, terminalibus. *Calycis* foliolis nonnullis quasi digitiformibus.

albicaule. *M.* (sharp-leaved white-twigged) foliis subulatis
9. viridibus semiteretibus, apice recurvulis mucronulatis; ramis gracilibus albicantibus.

Florebat in regio horto Kewense, Sept. 1825, &c. G. H. ♄.

Obs. Suffrutex suberectus gracilis subpedalis, facie *M. acuminati,* at 3-plo minus: vel *M. umbelliflori,* Willd. quod habet glauca parùm minora obtusa folia ramulosque rigidiores et fuscescentes. *Flores* (albi ut audivi) longè post florescentiam solùm vidi, neque examinavi.

In totâ Sectione, ramuli ex foliorum onere subindè terram versus decumbunt; et *folia* in juvenilibus plantis sæpè sulcata, sed in senectis ferè semper semicylindracea.

Sectio, TRICHOTOMA *Nob.* Suffrutices suberecti ramosi, foliis sæpè subcylindraceis, et ad solem micantibus atomis, calyce 5-phyllo floribus parvis ternatim 2—3-chotomis, colore vario, at sæpè cupreo-fulvicantibus.

macrorhizum. M. (white-flowered tuberous) foliis supernè
10. adunco-recurvis, ramis erectis loreo-flexilibus, radice magno tuberoso.

Florebat in regio horto Kewense A.D. 1824-5-6, per æstatem. G. H. ♄.

Obs. *M. tuberosum* maximè simulat, at differt præcipuè *caule* principi magìs æquali crassitudine, et non flexo, minùsque rigido, *ramis* magìs erectis crassioribus longéque flexilioribus s. loreis; *ramulis* numerosioribus s. confertioribus, cortice albidiore; *foliis* subduplò angustioribus acutioribus minùsque viridibus, s. magìs canescente-viridibus, *floribus* numerosioribus albis.

In *M. tuberoso, folia* supernè etiam adunco-recurva, *caudex* basi sursùm sensìm attenuatus; *radix* ætate maximè tuberosus semisuperterraneus; *rami* flexuosè patuli s. decumbentes subrigidi, ramulis paucioribus durioribus quàm in *M. macrorhizo,* non loreis; *flores* pauculi, cupreo-subfulvicantes, nec albi.

 XLIX. *On*

XLIX. *On the Use of the Blisterer.* By Sir ANTHONY CARLISLE, *Knt. F.R.S. &c.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

IN my letter to Sir Gilbert Blane upon blistering, &c., mentioned in your last Magazine, I have endeavoured to unite clearness with brevity; and it was my wish to distinguish a scald from a burn, to prevent practical mistakes in employing the heated metallic instrument.

The important difference between a scald and a burn is this. A simple scald arising from the momentary application of boiling water ought to be limited to that inflammatory excitement of the skin which produces a discharge of serum under the scarf skin, and this effect closely resembles a blister occasioned by cantharides;—but in the case of a burn, the higher degree or longer continuance of the heat corrugates the true skin, and thus its vascular structure is spoiled, and an eschar or a local detachment of the deadened part is the consequence.

When a mere blistering effect is desired, the operator must consider that the intervention of wetted silk presents the medium of water several degrees below the boiling point, as the agent to act upon the living skin; and if the duration of the contact resembles the sudden affusion of scalding water the results are similar. Your query as to the preferableness of common blisters because of their slow and longer continued action, demands an explanatory answer.—With every allowance for the diversity of medical opinions, I think the generality of experienced professional men must admit, that where counter irritation is required, the more sudden and intense the diversion, whether from the nervous or the sanguineous systems, the greater will be the power obtained, and thereby a more rational chance of relief from disorders of less degree. I apprehend that the remarkable cures effected by the moxa cautery depend much on the violence of its action, because in those instances where the metallic conductor of heat has acted as an escharotic the remedy has proved most efficacious.

A gentleman who had resided many years in Tanjore, informs me that the actual cautery is a very ancient remedy among the Hindus for diseases of cattle, and that a cow or an ox is rarely seen in that part of India without numerous scars of the cautery upon its body. The success of this practice is so firmly established, that the natives resort to the cautery whenever the hide of an animal appears to be tight or rough, and the application is usually made along the sides of the spine.

The

Fig. 6.

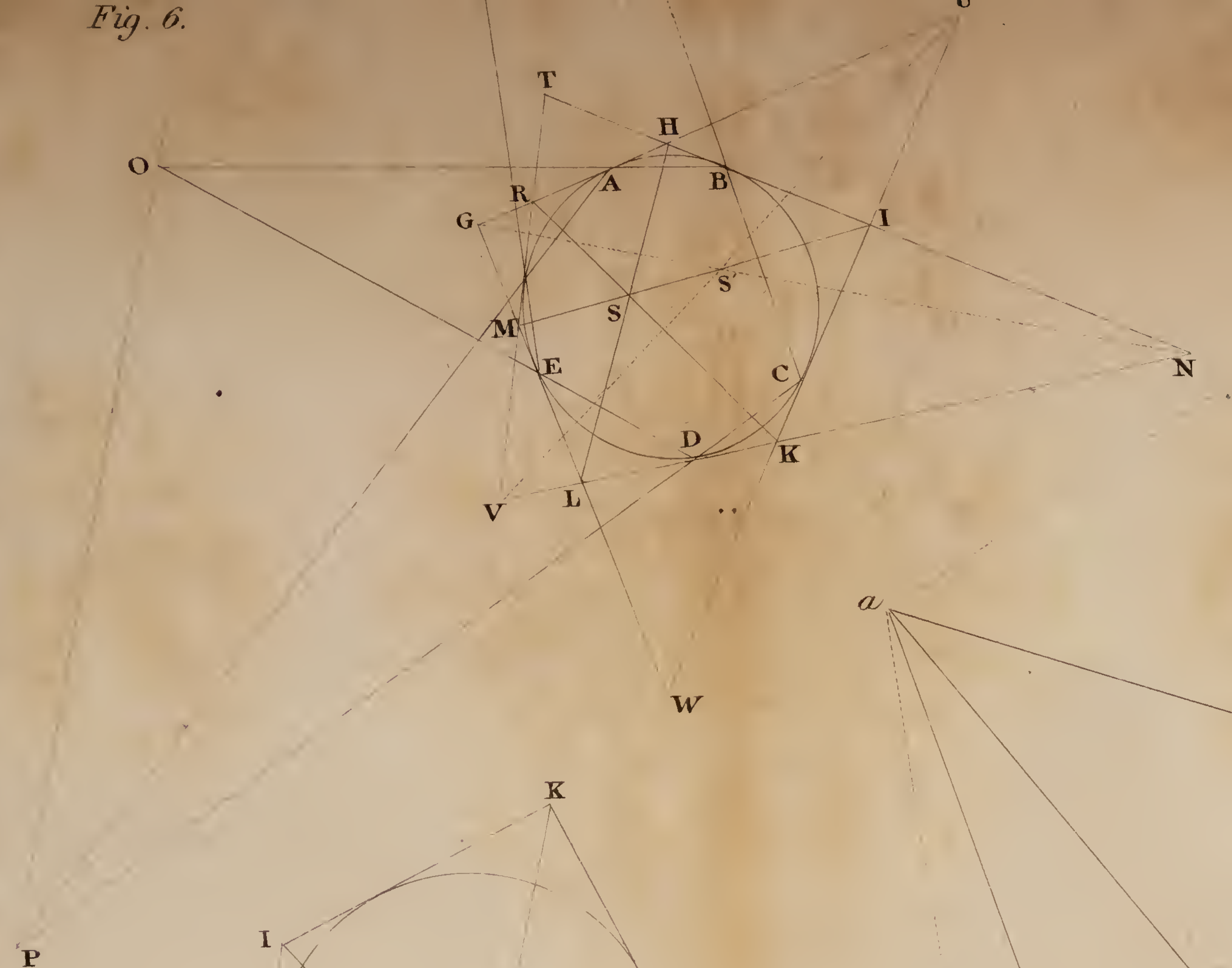
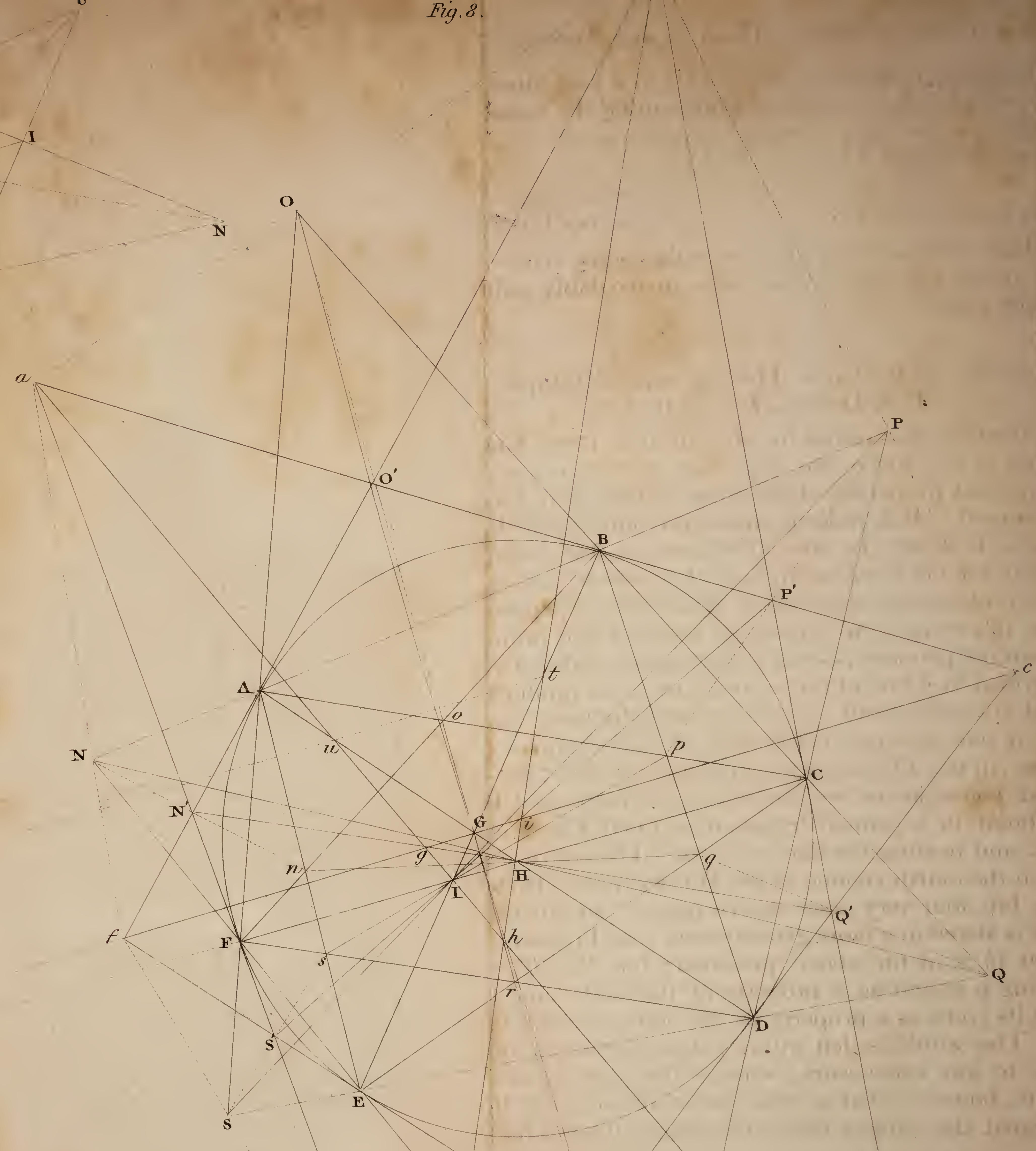


Fig. 8.



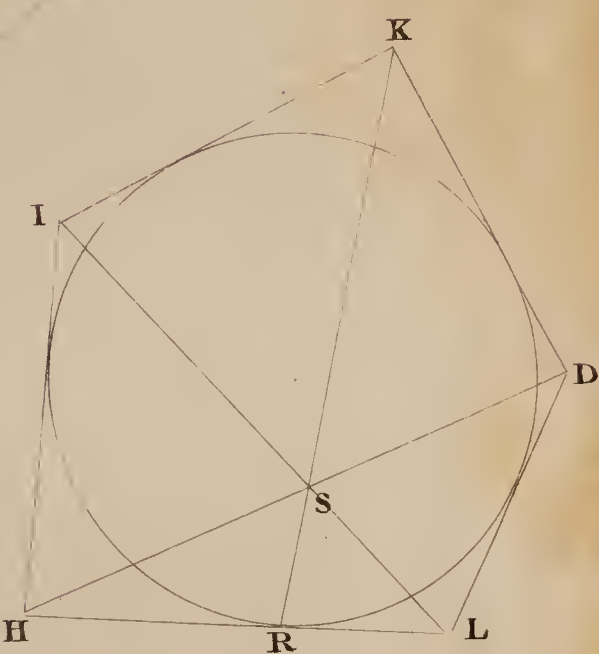


Fig. 7.

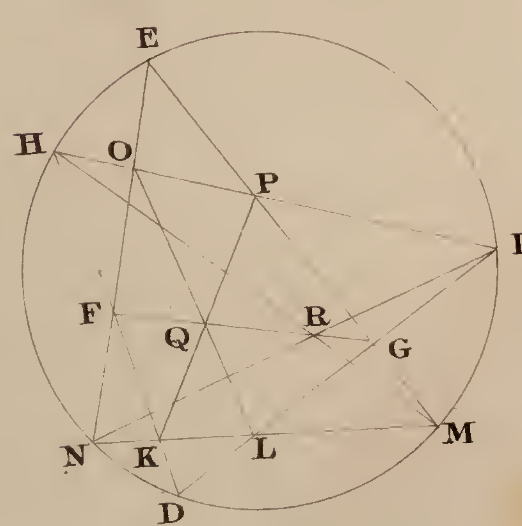


Fig. 5.

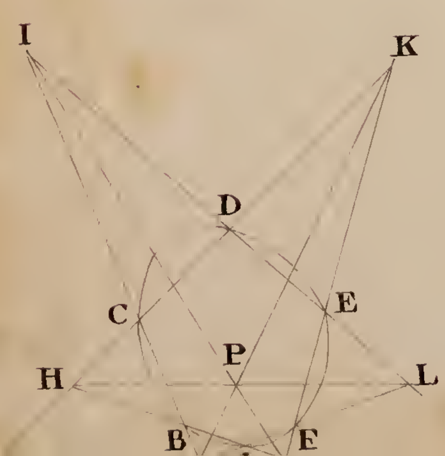


Fig. 4.

The presumed repugnance of patients to a heat-blister may be easily obviated by judiciously representing the momentary continuance of the pain, and the probable speedy alleviation of a serious disease.

Sir, your much obliged servant,

Langham Place, Nov. 2, 1826. ANTHONY CARLISLE.

P.S. I am obliged to you for correcting some errors in the printing of my pamphlet, which was unavoidably published without my revisal.

L. *Properties of* PASCAL'S Hexagramme Mystique. *By* T. S. DAVIES, *Esq. of Bath.**

THE theorem discovered by Maclaurin in 1722, and published at the end of his Algebra† in 1727, is one of the most important properties of the conic sections with which we are acquainted. It is, indeed, almost the only useful theorem we possess, in which the *axes*, *foci*, *centres*, and *diameters* of the sections are not introduced: and therefore it furnishes the means of investigating some of the most difficult properties of this class of curves. An immediate result of that principle is "the beautiful property,—that if the opposite sides of a hexagon inscribed in a line of the second order, be produced, the points of concourse will range in a straight line;" and was noticed by him amongst many other equally curious results in a paper in the Philosophical Transactions for 1733. This particular consequence was known to the celebrated Pascal, and is found in a printed fragment, entitled *Essais pour les Coniques*, and bearing the date of 1640. The entire paper is printed in the fourth volume of his Works (Paris, 1819), and occupies but four very open octavo pages. In this tract the property is stated in a more general form than Professor Leslie has given to it in the above quotation; but Pascal's way of considering it is first as a property of the circle, and thence inferring its truth as a property of the other sections, by *projection*. The whole is left without demonstrations, or even reference to any other work, whence they may be deduced. It appears, however, that at some period of his brief life, he had pursued the subject more into detail; though the tracts

* Communicated by the Author.

† Page 345, ed. 4. This theorem is comprised in a more general one of Brackenridge (Phil. Trans. 1735). A restricted case, viz. when all the poles are in a straight line, is in reality identical with the general theorem of Pappus, as restored by Simson (Phil. Trans. 1723, or *Tractatus de Porismatibus*, in his *Opera Reliqua*): whilst the theorem of Maclaurin under a similar restriction becomes virtually the same as the first of the said two general propositions.

were

were never printed, or have at all events been totally lost, as the Editor of his Works was never able to procure them either printed or in manuscript. Our only knowledge of their having existed is a letter of Leibnitz to M. Perrier, the nephew of Pascal, which is printed at the end of the fifth volume of the edition of his Works, before mentioned: and it seems from that letter, that he attached some importance to the inquiry, and perhaps some *occult qualities* to the figure, by the name which he conferred upon it, "*L'Hexagramme Mystique.*" But what were the properties which he discovered, cannot now be known; nor even whether he had examined all the cases to which the varied relations between the component parts of the hypothesis may give rise. The most probable conjecture we can form is, that his discoveries were those which most readily suggested themselves to us in an examination of the same figure. Those which follow are a few of the most obvious and interesting which occurred to me in studying the subject some time ago; and which I ought to add was before I had seen Pascal's Works. I had been more than once surprised to find that properties of the conic sections which were new in appearance were but particular cases of this singular theorem. I was thus led to examine its different cases, and the following form (virtually the same as Pascal's) was that which offered itself to my consideration.

Prop. I.

DE and FG are two chords in any conic section, which intersect in B; and F, I are two points taken at pleasure in the circumference of the section. Let EF, HI intersect in A, and DI, FG in C: then A, B, C are one straight line.

As B may be either within or without the conic section, two Classes of cases corresponding thereto will result.

It is unnecessary to insert the figures to this Prop. as the reader can sketch them himself.

Class I.—When B is *without* the conic section; and

When I is in the arc GH and F in the arc	EG	. . . 1
	GH	. . . 2
	HD	. . . 3
	DE	. . . 4
When I is in the arc DE and F in the arc	EG	. . . 5
	GH	. . . 6
	HD	. . . 7
	DE	. . . 8
When I is in the arc DH and F in the arc	EG	. . . 9
	GH	. . . 10
	HD	. . . 11
	DE	. . . 12

When

$$\text{When I is in the arc EG and F in the arc}\left\{\begin{array}{l}\text{EG}\\ \text{GH}\\ \text{HD}\\ \text{DE}\end{array}\right\}\begin{array}{l}\ldots 13\\ \ldots 14\\ \ldots 15\\ \ldots 16\end{array}$$

Class II.—When B is *within* the conic section; and

$$\text{When I is in the arc GD and F in the arc}\left\{\begin{array}{l}\text{EG}\\ \text{GD}\\ \text{DH}\\ \text{HE}\end{array}\right\}\begin{array}{l}\ldots 17\\ \ldots 18\\ \ldots 19\\ \ldots 20\end{array}$$

$$\text{When I is in the arc DH and F in the arc}\left\{\begin{array}{l}\text{EG}\\ \text{GD}\\ \text{DH}\\ \text{HE}\end{array}\right\}\begin{array}{l}\ldots 21\\ \ldots 22\\ \ldots 23\\ \ldots 24\end{array}$$

$$\text{When I is in the arc HE and F in the arc}\left\{\begin{array}{l}\text{EG}\\ \text{GD}\\ \text{DH}\\ \text{HE}\end{array}\right\}\begin{array}{l}\ldots 25\\ \ldots 26\\ \ldots 27\\ \ldots 28\end{array}$$

$$\text{When I is in the arc EG and F in the arc}\left\{\begin{array}{l}\text{EG}\\ \text{GD}\\ \text{DH}\\ \text{HE}\end{array}\right\}\begin{array}{l}\ldots 29\\ \ldots 30\\ \ldots 31\\ \ldots 32\end{array}$$

These appear to be all the variations which can take place, though Mr. Drummond, in his Demonstration of the converse property, states (Phil. Mag. No. 299.) the number of cases to be forty. But as that gentleman has not given any specific enumeration, it would become tedious to inquire into the source of his oversight.

This theorem (in the form of a property of the inscribed hexagon) has been so often demonstrated,—and as the demonstration of that case applies, *mutatis mutandis*, to all the others,—it is unnecessary to annex a new one here. A remark on one or two of the cases will be sufficient. Case 9. is that property of the inscribed hexagon so often mentioned: Case 21. is that figured in Maclaurin's Properties of Curve Lines, fig. 27; and Case 32. is that mentioned in my paper on the Trapezium, scholium to Prop. VI. It may seem superfluous to remark that from this theorem are also derived extremely simple demonstrations of the well-known properties of the inscribed and circumscribed trapezia found in many of our best treatises on Conics: and further, that if the rectilineal angle be considered (as Pascal considered it) one of the conic sections, the sixth proposition of my paper before alluded to, also becomes a specimen of the 32nd case enumerated above.

The first series of corollaries that follow from combining the different cases of this theorem are concerning the trapezium.

Cor.

Cor. 1.—The proposition numbered IX. in my former paper may be demonstrated in all its cases by binary combinations of the different cases of Pascal's theorem. The demonstration, however, there given being upon the principles of plane geometry is preferred to one in which the conic sections are involved.

Cor. 2.—Converse of Pascal's theorem: if any two lines (LI, HK) intersect each other in P in either diagonal (FG) of a trapezium EFDG and prolonged to cut the sides in four points L, K, H, R, a conic section will pass through E, L, R, D, I, H.

Cor. 3.—Conceive the lines HD, EK to meet in A′, and LD, EI in C′; then A′C′ will pass through B.

Also, if ER, LD be conceived to meet in *a*, and EI, DH in *b*, then *a* P *b* will be a straight line.

Cor. 4.—If the two lines LI, HK pass through the intersection of the diagonals of the trapezium, a conic section will pass through H, I, K, L, and either pair of opposite angles E, D or F, G.

It is obvious enough how to determine the *species* of the curve in every case that can arise.

Prop. II.

Let two triangles IGL, HKM mutually intersect each other in A, B, C, D, E, F, so that these points be posited in the circumference of any conic section: then lines IM, KG, HL, joining the opposite vertices, will intersect in the same point. (See Plate IV. fig. 4.)

Dem.—Prolong each pair of opposite sides of the hexagon, ABCDEF to meet in N, O, P: then (Prop. I.) N, O, P will be in a straight line.

Again, because N, O, P are in one straight line, the lines MI, KG intersect in the line HP by Prop. IX. (Trapezium, Phil. Mag. No. 340.) Q. E. D.

Cor. 1.—Prolong the alternate sides of an inscribed hexagon till they meet: the three diagonals giving the three pairs of opposite intersections will meet in a point. This is only another mode of stating the above property.

Cor. 2.—If three lines MI, GK, HL cross each other in the same point P, and two triangles IGL, HKM be described, the vertices of which are in these three lines, then the six intersections of these two triangles are in the periphery of a conic section.

Prop. III.

Let two triangles HID, NEM be inscribed in a conic section, and by their mutual intersections form a hexagon of which

which the diagonals are OL, FG, PK: these will pass through the same point Q. (See fig. 5.)

Dem.—Since (Prop. I. case 32. is suited to the figure) the points F, R, Q are in a straight line, it follows (by Prop. VII. Properties of Trapezium, Phil. Mag. No. 340.) that OL, KP also intersect in FG. Q. E. D.

Cor. 1.—Conversely, if the angles of a hexagon be situated upon the lines which pass through the same point, the alternate sides being prolonged will meet in six points through which a conic section will pass.

Prop. IV.

The three diagonals HL, IM, RK of a hexagon circumscribing a conic section pass through the same point S. (See fig. 6.)

Dem.—Draw BA, DE to meet in O, AF, CD to meet in P, and FE, BC to meet in Q: then (Prop. I.) O,P,Q are in the same straight line.

Now, it is well known that whilst the locus of O is a straight line, the diagonal HL passes through a certain point S. And hence it follows, since P,Q are in the same straight line with O, the diagonals IM and RK also pass through the same point S. Q. E. D.

Cor. 1.—If the three diagonals of a hexagon pass through the same point, a conic section which touches five of the sides will also touch the sixth.

Cor. 2.—If two triangles touch a conic section the lines joining the three pairs of opposite intersection will pass through the same point.

Cor. 3.—If two points of contact coalesce as A and F, the point R will also coalesce with them, and the figure is converted into a pentagon. From which we derive an easy method of describing a trajectory to touch five lines given in position. (Newt. *Princip.* lib. i. sect. 5. pr. 27.) Draw the diagonals (fig. 7.) LI, HD to intersect in S; the line KS being drawn to cut the opposite side in R, one of the points of contact. In the same manner may the other points of contact be found, and the section may then be described as usual.

Cor. 4.—Let HGNL be a trapezium circumscribing a conic section; and IK, RM tangents, each cutting one of the triangles into which the diagonal HL divides the triangles in I, K, R, M; then RK, IM will intersect in HL.

Cor. 5.—The lines UV, NG intersect also in the diagonal MI; and the same of the others. This corollary is, properly speaking, a case of the fourth: viz. when the *prolongations* of the sides of the trapezium are cut by the two tangents.

PROP. V.

Let two triangles OSQ, NPR intersect each other in a conic section in A, B, C, D, E, F (as in Prop. II.); let *ace*, *bfd'* be the triangles whose contacts with the conic section are A, B, C, D, E, F, and N', O', P', Q', R', S be their mutual intersections (as in Prop. IV.); and, lastly, let *n*, *o*, *p*, *q*, *r*, *s* be the intersections of the sides of the two inscribed triangles (as in Prop. III.) See fig. 8. Then,

1°. The six lines PS, P'S', *p s*, *a d*, AD, and EP will pass through the same point, I; the six NQ, N'Q', *n q*, *b e*, AD, and FC will pass through another point H; and the six OR, O'R', *o r*, *c f*, EP and FC will pass through a third point G.

2°. The intersections of *a d*, *b e*, *f c* will be situated in the three diagonals S'P', N'Q', O'R'.

Dem.—First, the line S'P' is one of the diagonals of a tangential trapezium, and FC, EB the diagonals of the inscribed one: therefore S'P' passes through their intersection I.

Also by Prop. I. (cases 21. and 32. in reference to this diagram), SP and *s p* pass through I.

Again, (by Prop. IV. cor. 4.) the line *a d* also passes through I.

In a similar manner the other two clusters of lines are proved to pass through H and G. Q. E. 1moD.

Secondly, It is clear also from Prop. IV. cor. 4. that *a d*, *f c* intersect in N'Q'; *a d*, *b e* in S'P'; and *b e*, *f c* in O'R'. When &c. Q. E. 2° D.

Cor. 1.—A conic section will touch all the sides of the *hexagramme*, *n o p q r s*; another may be drawn to touch the *hexagramme* NOPQRS.

Cor. 2.—Draw NOPQRS to meet alternately; the six points of section will be in a line of the second order: and the points N', O', P', Q', R', S' will be in another.

It is obvious that these processes may be extended indefinitely within and without the given conic section ABCDEF.

Cor. 3.—The points P, P', *p* are in one straight line; and the same is true of Q, Q', *q*; R, R', *r*; &c.

For in this case, two angles of the *hexagram* have coincided with two others, so that two of its sides are represented by the tangents BP', CP'; whence this is a specimen of case 11th of Prop. I. and the truth of the corollary is obvious.

Cor. 4.—The points *a*O*b* are in one straight line; and the same is true of *b*, P, *c*; *c*Q*d*; &c.

For it is well known that the opposite sides of the tangential trapezium N'O'P'R' meet in the same line as the opposite sides of the inscribed trapezium ABCF.

Cor.

Cor. 5.—Let the three diagonals of the inscribed hexagon meet the three diagonals joining the opposite summits of the tangential triangles in t, u, v; these three points are in one straight line.

Several theorems analogous to these have been omitted on account of the extreme complication of their respective diagrams.

Bath, Sept. 1826.

LI. *An Account of a new Catalogue of Stars, recently published by the Astronomical Society of London.*

THIS catalogue contains the mean places (reduced to January 1, 1830) of nearly three thousand principal stars: together with the logarithms of certain *constants* for determining their precession, aberration and nutation; and by the help of which, those quantities may be computed with much greater ease and expedition than by any other method hitherto made known. An introduction is prefixed to the tables, by Mr. Baily, President of the Society, explanatory of their construction and use: and from which we select the following passages:

"Special tables, for computing the aberration and nutation of particular stars, have for a long time been used by astronomers. The first distinct publication of this kind was by M. Mezger; who published at Manheim in 1778, his *Tabulæ Aberrationis et Nutationis* for 352 stars. There had, however, previously to that period, appeared in the volumes of the *Connaissance des tems* from 1760 to 1774, several tables of a similar kind, and containing many of the same stars: which tables, M. Jeaurat subsequently collected together, and published in the *Con. des tems* for 1781. They were afterwards revised by M. Delambre, and published (252 in number) in the *Con. des tems* for 1789—1791. An addition of 116 stars was made in the *Con. des tems* for 1802; and a further addition of 142 stars, in the same work for 1806: thus making the total number 510. In the *Ephemerides de Vienne* for the years 1784 and 1785, M. Pilgram published special tables for 500 stars: but they are said to contain so many errors that it is unsafe to use them. In the year 1807, two other sets of special tables appeared, comprising nearly the same stars as those already alluded to: one by M. Cagnoli, containing 501 stars: the other by Baron Zach, containing 494 stars. The former is entitled *Catalogue de 501 etoiles, suivi des tables relatives*

d'Aberration et de Nutation: Modena, 1807. And the latter, *Tabulæ Speciales Aberrationis et Nutationis,* &c. Gotha, 1807: 2 vols. octavo. In this last-mentioned work, the second volume only is devoted to the tables of aberration and nutation; and each star occupies a whole page. The first volume contains much useful information connected with the same subject, and many other valuable tables.

"Hitherto the attention of astronomers had been confined to about five hundred of the principal stars: and in this state the subject remained till the year 1812, when some new tables, differently constructed and of a more general kind, were published by Baron Zach. These are the most comprehensive as well as the most convenient set of tables, which have hitherto been formed for such computations. They are entitled *Nouvelles tables d'Aberration et de Nutation pour* 1440 *etoiles;* and were published at Marseilles in 1812, in one volume octavo. But, in these tables, the solar nutation, as well as some other minute quantities, are wholly omitted: and although that celebrated author has given a rule (in page 26) whereby we may approximate to the value of the solar nutation, yet that rule is not strictly correct, and ought not to be resorted to in the present state of the science.

"I would observe, that when we wish to compute the aberration and nutation by the tables of Baron Zach, here alluded to, it is necessary to form distinct arguments for the sines of the quantities employed; the logarithms of which quantities must be sought for, and taken out of a book of logarithms. And, for the purpose of forming the arguments, reference must be made to some ephemeris; and certain proportional parts must be computed before a correct solution can be obtained. We have then to obtain the sums of four logarithms, and to find the natural numbers corresponding thereto. After this, we have to compute the precession and solar nutation for the given day, by a separate calculation of no little trouble, before we can deduce the total correction. Those only, required who are versed in such calculations, can fully appreciate the labour, the risk of error, and the loss of time concerned in these several operations.

"By the method, however, which I am about to explain, nearly the whole of this troublesome process may be saved. For, in most ordinary cases, it will not be necessary to form any argument, nor in any case to refer to an ephemeris, or any other work, except to a *small* table of logarithms. We have merely to add four logarithms found in one of the present tables, to four logarithms found in another of those tables, and

and the natural numbers, corresponding to the sums of those logarithms, will give the whole correction, either in right ascension or declination, as may be required; and with a degree of accuracy not previously attempted.

"The mode by which this great saving of time and labour is obtained, has been, in some measure, already explained by me in the Philosophical Magazine for October 1822; and the plan, which was first published by Professor Bessel, has been partially acted on by Professor Schumacher in his *Astronomische Hülfstafeln* for the same year. The tables of M. Schumacher, however, comprehend little more than two-thirds of the *first* class, only, of the stars to which I am about to allude; and do not exceed five hundred in number.

"The stars, which form the subject of the present tables, consist of the three following classes:

"1°. All the stars, to the *fifth* magnitude inclusive, wheresoever situated.

"2°. All the stars, to the *sixth* magnitude inclusive, situated within 30° of the equator.

"3°. All the stars, to the *seventh* magnitude inclusive, situated within 10° of the ecliptic.

"This selection, it is presumed, will form one of the most useful catalogues for practical astronomy that can well be suggested. It contains *all* the stars of the above description, which have been observed (with sufficient accuracy for determining their present positions) by Flamsteed, Bradley, La Caille, Mayer, Piazzi, and Zach."

Mr. Baily then proceeds to explain the mode by which the stars have been brought up by precession; and likwise the method of forming the general tables of precession, aberration, and nutation: illustrating the same by suitable examples.

When the catalogue was finished, it became desirable to ascertain how far the mean places of the stars (which had been brought up from the observations of Bradley and Piazzi by means of the proposed formula) could be depended upon. With this view a comparison was made with the places of the 36 Greenwich stars that have been observed and reduced at different times by Messrs. Bessel, Brinkley, and Pond: and which is inserted at the end of the work.

There are two Catalogues of the Right Ascension of the 36 Greenwich stars published by M. Bessel in the Konigsberg Observations: one (which may be considered as Dr. Maskelyne's catalogue of 1805) reduced to 1815, and the other (depending on M. Bessel's own observations) reduced to 1825. Both these catalogues were brought up to 1830 by means of the

the annual variations attached to the catalogue of 1825. The catalogue of the same stars by Dr. Brinkley was taken from M. Schumacher's *Astronomische Nachrichten*, No. 78: it is there reduced to 1824, but was brought up to 1830 by means of the annual variations annexed thereto. The *first* catalogue, in Æ, of Mr. Pond, was taken from that (reduced to 1819) which is inserted in the Nautical Almanac for 1822; and was the last that was published prior to his important alteration of the position of the equinoctial points by the additiou of $0^{s}{\cdot}31$ to all the stars. The *second* catalogue of Mr. Pond was that (reduced to 1825) which is published in the Nautical Almanac for 1829, and contains the latest corrections, to August 1826. Both these catalogues were brought up to 1830 by means of the annual variations annexed to the *latter* catalogue.

On a comparison of these several catalogues it appears that, as to the *Right Ascensions*, the catalogue of the Astronomical Society falls far within the limits of the errors of observation: since more than two-thirds of the stars there compared are *between* the mean places as severally given by these eminent observers: and in those instances where this is not the case, the position does not differ so much from that of some one of the observers, as those observers do from each other, and from themselves.

With respect to the *North Polar Distances* recourse was had to the two catalogues of Mr. Pond: one reduced to 1818, (being the last correction of his Standard Catalogue of 1812-13, prior to the derangement of the mural circle) and published in the Nautical Almanac for 1820: the other reduced to 1825, and taken from the Nautical Almanac for 1829, above mentioned. These were brought up to 1830 by means of the annual variations annexed to the latter catalogue: and which differ, in some instances very considerably, from the values annexed to the catalogue of 1818. Out of the 70 comparisons made, it will be found that in nearly one half of them the difference is below one second; that in 16 others the difference is below two seconds; and that in 7 others the difference is below three seconds: whilst the difference in the remainder (which in five cases, only, exceeds four seconds) may be considerably reduced by the adoption of the annual variations annexed to the catalogue of 1818; the difference of which will in fact, in many of the comparisons, amount to a quantity equal to the whole of the difference in question. Indeed, a difference in the *mode of reduction* will frequently account for differences, as great as any that occur in these comparisons: as may be seen in our *Journal* for July 1825, page 33.

The

The *mean difference* of each catalogue from that of the Astronomical Society is inserted at the bottom of the respective columns: and will be found as follows:

Bessel,	1815	$= - 0^{s}\cdot004$	Right Ascension.
——	1825	$= + 0\cdot151$	
Brinkley,	1824	$= + 0\cdot017$	
Pond,	1819	$= + 0\cdot023$	
——	1825	$= + 0\cdot351$	
Pond,	1818	$= + 0''\cdot64$	North Polar Distance.
——	1825	$= + 1\cdot64$	

LII. *Additional Experiments and Observations on the Strength of Timber.* *By* B. Bevan, *Esq.*

To the Editor of the Philosophical Magazine and Journal.

Sir,

To the Table published in your last Number, page 270, may be added the following, which have since been submitted to experiment:

	Spec. Gr.	lbs. Cohesion per square inch.
Apple	·71	19·500
Elm	·69	14·400
Hazle	·86	18·000 +
Hornbeam	·82	20·240 +
Larch	·57	8·900 —
Plane	·64	11·700 —

Nearly all the species of wood submitted to longitudinal strain, for obtaining the force of cohesion per square inch, by the direct process, were also subjected to transverse fracture, by a load applied to the middle of a transverse bar when supported at each end.

Let l = length; b, breadth; d, depth of the prism in inches;
w = the weight in pounds applied on the middle;
c = the cohesion per square inch in pounds.

If the resistance to compression were equal to that of extension
$$\frac{1\cdot5\, lw}{b\, d^2} = c$$
the mean result of my experiments give for dry and seasoned wood
$$\frac{2\, l\, w}{b\, d^2} = c$$
and when the wood is green, unseasoned, or wet,
$$\frac{2\, l\, w}{b\, d^2} = c$$

These

These formulæ will be found of considerable use to the practical engineer and architect.

I am aware that the degree of flexure of the specimen will in a small measure affect the result, but too small to deserve attention in practice. Yours truly,

B. Bevan.

LIII. *On the Existence of a Limit to Vaporization. By* M. Faraday, *F.R.S. Corresponding Member of the Royal Academy of Sciences at Paris, &c. &c.**

It is well known that within the limits recognised by experiment, the constitution of vapour † in contact with the body from which it rises, is such, that its tension increases with increased temperature, and diminishes with diminished temperature; and, though in the latter case we can, with many substances, so far attenuate the vapour as soon to make its presence inappreciable to our tests, yet an opinion is very prevalent, and I believe general ‡, that still small portions are produced; the tension being correspondent to the comparatively low temperature of the substance. Upon this view, it has been supposed that every substance *in vacuo* or surrounded by vapour or gas, having no chemical action upon it, has an atmosphere of its own around it; and that our atmosphere must contain, diffused through it, minute portions of the vapours of all those substances with which it is in contact, even down to the earths and metals. I believe that a theory of meteorites has been formed upon this opinion.

Perhaps the point has never been distinctly considered; and it may therefore not be uninteresting to urge two or three reasons, in part dependent upon experimental proof, why this should not be the case. The object, therefore, which I shall hold in view in the following pages, is to show that a *limit* exists to the production of vapour of any tension by bodies placed *in vacuo*, or in elastic media, beneath which limit they are perfectly fixed.

Dr. Wollaston, by a beautiful train of argument and observation, has gone far to prove that our atmosphere is of finite extent, its boundary being dependent upon the opposing powers of elasticity and gravitation §. On passing upwards,

* From the Philosophical Transactions, for 1826. Part III.

† By the term vapour, I mean throughout this paper that state of a body in which it is permanently and indefinitely elastic.

‡ See Sir H. Davy's paper On Electrical Phænomena exhibited *in vacuo*. Phil. Trans. 1822, p. 70.

§ Thil. Trans. 1822, p. 89.

from

from the earth's surface, the air becomes more and more attenuated, in consequence of the gradually diminishing pressure of the superincumbent part, and its tension or elasticity is proportionally diminished; when the diminution is such, that the elasticity is a force, not more powerful than the attraction of gravity, then a limit to the atmosphere must occur. The particles of the atmosphere there tend to separate with a certain force; but this force is not greater than the attraction of gravity, which tends to make them approach the earth and each other; and as expansion would necessarily give rise to diminished tension, the force of gravity would then be strongest, and consequently would cause contraction, until the powers were balanced as before.

Assuming this state of things as proved, the air at the limit of the atmosphere has a certain degree of elasticity or tension; and, although it cannot there exist of smaller tension, yet, if portions of it were removed to a further distance from the earth, or if the force of gravity over it could in any other way be diminished, then it would expand, and exist of a lower tension; upon the renewal of the gravitating force, either by approximation to the earth's surface or otherwise, the particles would approach each other, until the elasticity of the whole was again equal to the force of gravity.

Inasmuch as gases and vapours undergo no change by mere expansion or attenuation, which can at all disturb the analogy existing between them in their permanent state under ordinary circumstances, all the phænomena which have been assumed as occurring with the air at the limit of our atmosphere may, with equal propriety, be admitted with respect to vapour in general in similar circumstances; for we have no reason for supposing that the particles of one vapour more than another are *free* from the influence of gravity, although the force may, and without doubt does, vary, with the weight and elasticity of the particles of each particular substance.

It will be evident, also, that similar effects would be produced by the force of gravity upon air or vapour of the extreme tenuity and feeble tension referred to, whatever be the means taken to bring it into that state; and it is not necessary to imagine the portion of air operated upon, as taken from the extremity of our atmosphere, for a portion of that at the earth's surface, if it could be expanded to the same degree by an air-pump, would undergo the same changes: when of a certain rarity it would just balance the attraction of gravitation and fill the receiver with vapour; but then, if half were taken out of the receiver, the remaining portion, in place of filling the vessel, would submit to the force of gravity, would con-

tract into the lower half of the receiver, until, by the approximation of their particles, the vapour there existing should have an elasticity equal to the force of gravity to which it was subject. This is a necessary consequence of Dr. Wollaston's argument.

There is yet another method of diminishing the elasticity of vapour, namely, by diminution of temperature. With respect to the most elastic substances, as air, and many gases, the comparatively small range which we can command beneath common temperatures, does nothing more at the earth's surface than diminish in a slight degree their elasticity, though two or three of them, as sulphurous acid and chlorine, have been in part condensed into liquids. But with respect to innumerable bodies, their tendency to form vapour is so small, that at common temperatures the vapour produced approximates in rarity to the air upon the limits of our atmosphere; and with these, the power we possess of lessening tension by diminution of temperature, may be quite sufficient to render it a smaller force than its opponent, gravity; in which case it will be easy to comprehend that the vapour would give way to the latter, and be entirely condensed. The metal, silver, for instance, when violently heated, as on charcoal urged by a jet of oxygen, or by the oxy-hydrogen, or oxy-alcohol flame, is converted into vapour: lower the temperature, and before the metal falls beneath a white heat, the tension of the vapour is so far diminished, that its existence becomes inappreciable by the most delicate tests. Suppose, however, that portions are formed, and that vapour of a certain tension is produced at that temperature; it must be astonishingly diminished by the time the metal has sunk to a mere red heat; and we can hardly conceive it possible, I think, that the silver should have descended to common temperatures, before its accompanying vapour will, by its gradual diminution in tension, if uninfluenced by other circumstances, have had an elastic force far inferior to the force of gravity; in which case, that moment at which the two forces had become equal, would be the last moment in which vapour could exist around it; the metal at every lower temperature being perfectly fixed.

I have illustrated this case by silver, because, from the high temperature required to make any vapour appreciable, there can be little doubt, that the equality of the gravitating and elastic forces, must take place much above common temperatures, and therefore within the range which we can command. But there is, I think, reason to believe that the equality in these forces, at or above ordinary temperatures, may take place with bodies far more volatile than silver; with sub-

stances

stances indeed which boil under common circumstances at 600° or 700° F.

If, as I have formerly shown*, some clean mercury be put at the bottom of a clean dry bottle, a piece of gold leaf attached to the under part of the stopper by which it is closed, and the whole left for some months at a temperature of from 60° to 80°, the gold leaf will be found whitened by amalgamation, in consequence of the vapour which rises from the mercury beneath; but upon making the experiment in the winter of 1824-5, I was unable to obtain the effect, however near the gold leaf was brought to the surface of the mercury; and I am now inclined to believe, because the elastic force of any vapour which the mercury could have produced at that temperature, was less than the force of gravity upon it, and that consequently the mercury was then perfectly fixed.

Sir Humphry Davy, in his experiments on the electrical phænomena exhibited *in vacuo,* found, that when the temperature of the vacuum above mercury was lowered to 20° F. no further diminution, even down to −20° F. was able to effect any change, as to the power of transmitting electricity, or in the luminous appearances; and that these phænomena were then nearly of the same intensity as in the vacuum made over tin†. Hence, in conjunction with the preceding reasoning, I am led to conclude, that they were then produced independent of any vapour of the metals, and that under the circumstances described; no vapour of mercury existed at temperatures beneath 20° F.

Concentrated sulphuric acid boils at about 600° F.; but as the temperature is lowered the tension of its vapour is rapidly diminished. Signor Bellani ‡ placed a thin plate of zinc at the upper part of a closed bottle, at the bottom of which was some concentrated sulphuric acid. No action had taken place at the end of two years, the zinc then remaining as bright as at first; and this fact is very properly adduced in illustration of the fixedness of sulphuric acid at common temperatures. Here I should again presume, that the elastic force which tended to form vapour, was surpassed by the force of gravity.

Whether it be admitted or not, that in these experiments the limit of volatilization, according to the principle of the balance of forces before stated had been obtained, I think, we can hardly doubt that such is the case at common temperatures, with respect to the silver, and with all bodies which bear a high temperature without appreciable loss by volatilization,

* Quarterly Journal of Sciences, x. 354.

† Phil. Trans. 1822, p. 71.

‡ *Giornale di Fisica,* v. 197.

zation, as platina, gold, iron, nickel, silica, alumina, charcoal, &c.; and, consequently that, at common temperatures, no portion of vapour rises from these bodies or surrounds them; that they are really and truly fixed; and that none of them can exist in the atmosphere in the state of vapour.

But there is another force, independent of that of gravity, at least of the general gravity of the earth, which appears to me sufficient to overcome a certain degree of vaporous elasticity, and, consequently, competent to the condensation of vapour of inferior tension, even though gravity should be suspended; I mean the force of homogeneous attraction.

Into a clean glass tube, about half an inch in diameter, introduce a piece of camphor; contract the tube at the lamp about four inches from the extremity; then exhaust it, and seal it hermetically at the contracted part; collect the camphor to one end of the tube; and then, having placed the tube in a convenient position, cool the other end slightly, as by covering it with a piece of bibulous paper preserved in a moist state by a basin of water and thread of cotton; in this way, a difference in temperature of a few degrees will be occasioned between the ends of the tube, and after some days, or a week or two, crystals of camphor will be deposited in the cooled part; there will not, however, be more than three or four of them, and these will continue to increase in size as long as the experiment is undisturbed, without the formation of any new crystals, unless the difference of temperature be considerable.

A little consideration will, I think, satisfy us that, after the first formation of the crystals in the cooled part, they have the power of diminishing the tension of the vapour of camphor, below that point at which it could have remained unchanged in contact with the glass, or in space: for the vapour of the camphor is of a certain tension in the cooled end of the tube, which it can retain in contact with the glass, and therefore it remains unchanged; but which it cannot retain in contact with the crystal of camphor, for there it is condensed, and continually adds to its mass. Now, this can only be in consequence of a positive power in the crystal of camphor of attracting other particles to it; and the phænomena of the experiment are such as to show, that the force is able to overcome a certain degree of elasticity in the surrounding vapour. There is therefore no difficulty in conceiving that, by diminishing the temperature of a body and its atmosphere of vapour, the tension of the latter may be so far decreased, as at last to be inferior to the force with which the solid portion, by the attraction of aggregation, draws the particles to it; in which

which case it would immediately cause the entire condensation of the vapour.

The preceding experiments may be made with iodine, and many other substances; and indeed there is no case of distinct crystallization by sublimation* which does not equally afford evidence of the power of the solid matter, to overcome a positive degree of tension in the vapour from which the crystals are formed. The same power, or the force of aggregation, is also illustrated in crystallizing solutions; where the solution has a tendency to deposit upon a crystal, when it has not the same tendency to deposit elsewhere.

It may be imagined that crystallization would scarcely go on from these attenuated vapours, as it does in the denser states of the vapours experimented upon. There is, however, no good reason for supposing any difference in the force of aggregation of a solid body, dependent upon changes in the tension of the vapour about it; and indeed, generally speaking, the method I have assumed for diminishing the tension of the vapour, namely, by diminishing temperature, would cause increase in the force of aggregation.

Such are the principal reasons which have induced me to believe in the existence of a limit to the tension of vapour. If I am correct, then there are at least two causes, each of which is sufficient to overcome and destroy vapour when reduced to a certain tension; and both of which are acting effectually with numerous substances upon the surface of the earth, and retaining them in a state of perfect fixity. I have given reasons for supposing that the two bodies named, which boil at about 600° F. are perfectly fixed within limits of low temperature which we can command; and I have no doubt, that, nearly all the present recognised metals, the earths, carbon, and many of the metallic oxides, besides the greater number of their compounds, are perfectly fixed bodies at common temperatures. The smell emitted by various metals when rubbed may be objected to these conclusions, but the circumstances under which these odours are produced, are such, as not to leave any serious objections on my mind to the opinions above advanced.

I refrain from extending these views, as might easily be done, to the atomic theory, being rather desirous that they should first obtain the sanction or correction of scientific men. I should have been glad to have quoted more experiments upon the subject, and especially relative to such bodies as acquire their fixed point at, or somewhat below, common temperatures.

* Calomel, corrosive sublimate, oxide of antimony, naphthaline, oxalic acid, &c. &c.

peratures. Captain Franklin has kindly undertaken to make certain experiments for me in the cold regions to which he has gone; and probably when he returns from his arduous undertaking, he may have some contributions towards the subject.

Royal Institution, May 4, 1826.

LIV. *Short Abstract of* M. de Freycinet's *Experiments for determining the Length of the Pendulum. By* James Ivory, *Esq. M.A. F.R.S.**

SINCE my former communication in the beginning of the month, I have been favoured with the perusal of that part of M. de Freycinet's work which treats of his experiments with the pendulum. In order to complete the view of the present state of this subject, I have hastily drawn up the following abstract of the principal results obtained by the labours of M. de Freycinet. The facts of the new experiments are contained in the following table:

Station.	Latitude.	Pendulum. Relative lengths.	Pendulum. Inches.
Falkland Islands . .	51° 35′ 18″ S.	1·00022319	39·13712
Cape of Good Hope	33 55 15	·99871582	39·07817
Port Jackson	33 51 34	·99877424	39·08046
Rio Janeiro	22 55 13	·99783538	39·04371
Isle of France	20 9 56	·99794215	39·04769
Rawak†	0 1 34	·99709575	39·01479
Guam‡.	13 27 51 N.	·99759331	39·03023
Mowi§.	20 52 7	·99792816	39·04737
Paris.	48 50 14	1·00002271	39·12929

In this table the relative lengths are the pendulums beating seconds reduced to the level of the sea at the several stations, the seconds pendulum at the observatory at Paris being the unit. Taking 29·12929 in. for the length of the seconds pendulum at Paris at the level of the sea and at the standard temperature of 62° Fahr. according to the determination of MM. Biot, Mathieu and Bouvard, the other column of the table contains the pendulums of the several stations in English inches at the standard temperature.

Besides the relative lengths in the foregoing table, which are those definitely obtained, p. 26 of M. de Freycinet's book, there is another set given in the equations of condition at

* Communicated by the Author. † Island on the coast of N. Guinea.
‡ One of the Ladrones. § One of the Sandwich Islands.

p. 28.

p. 28. But of these latter numbers no explanation is any where to be found, neither are they ever referred to, nor used in any calculation. Two of these equations of condition are indeed set down at the bottom of p. 28, but this must be an inadvertence; for the calculation which is repeated at p. 229, is performed with the lengths in p. 26. It would however be very satisfactory to know upon what grounds the relative lengths in p. 28 were computed, and why they were rejected, because in general they agree better with the experiments of others than the lengths definitely adopted by the author.

If now, by means of the data in the foregoing table, we compute the lengths of the pendulums at the several stations by my formula in the last number of this Journal, we shall obtain as follows:

	Computed Pendulum.	Excess of Calculation.
Falkland Islands . .	39·13960	+·00248
Cape of Good Hope	39·07648	—·00169
Port Jackson	39·07637	—·00409
Rio Janeiro	39·04336	—·00035
Isle of France. . . .	39·03646	—·01121
Rawak.	39·01170	—·00309
Guam	39·02301	—·00722
Mowi	39·03816	—·00921

The inspection of this table shows that the pendulum at Rio Janeiro is the same by the experiments of M. de Freycinet as by those of Captain Hall and Foster. At the Falkland Islands M. de Freycinet makes the pendulum shorter than M. Duperrey; the error is greater, but it still falls within the limits of my table. The Cape of Good Hope is a new station to be added to my table; and the same may be said of the Island Rawak, the error in this latter case falling upon the extreme limit. We have therefore now 28 independent experiments which concur, with very small discrepancies, to give the same figure to the earth.

At Port Jackson there is evidently a considerable irregularity; for, according to the latitude, the pendulum should be shorter than at the Cape of Good Hope, whereas it is longer ·00229 in. This station is on the same main land with Paramatta, and indeed in the vicinity of it, the difference of longitude being about 14′, and the difference of latitude no more than 3′. We have two determinations of the pendulum at Paramatta; one by Sir T. Brisbane, which makes it 39·07696; and the other by Mr. Dunlop, of which the result is 39·07751*; and, as the addition for the small difference of latitude is in-

* Phil Trans. 1824.

significant,

significant, the same numbers, about ·003 less than according to M. de Freycinet, should express the length of the pendulum at Port Jackson. We have likewise a direct determination of the pendulum at this station by the Spanish navigators, whose experiments are calculated by M. Mathieu in the *Con. des Tems*, 1816 *; from which the length comes out equal to 39·07682, or ·00364 less than M. de Freycinet's experiment. We must therefore be allowed to demur a little with respect to the accuracy of the pendulum at Port Jackson; for according to what has been said, we cannot in this instance safely apply the usual remedy of local attraction.

At the Isle of France and at Guam and Mowi, the errors appear enormous. But it ought to be observed, that these errors are not the creations of my formula, which, in reality, represents the actual experiments with the same correctness as if the errors had been very small. If the pendulum beating seconds at the Isle of France be transported to Rio Janeiro, in the same hemisphere and nearly on the same parallel, it must be shortened, according to the experiments, ·00398 in. in order to oscillate in the same time; but, according to the difference of latitude, it should be lengthened about ·00657 in any hypothesis of ellipticity; wherefore there is really a local irregularity at the Isle of France compared with other points of the same parallel, affecting the length of the pendulum to the amount of ·01080, nearly the same as the error by my formula. And the same thing may be shown with regard to Guam and Mowi, by comparing the first with Madras and the other with San Blas. The purpose of a formula strictly deduced from facts without any undue aid from hypothesis is, not to extinguish discrepancies actually existing in Nature, or supposed so to exist, but to exhibit them as they really are.

On the whole, it does not appear that the experiments of M. de Freycinet bring into notice any good reasons for changing our opinion about the mean figure of the earth, whatever they may do with regard to great irregularity in the distribution of gravity.

In the Annals of Philosophy for Oct. and Nov., there is an account of an experimental determination of the pendulum at the equator by Captain Goldingham. It must be remarked that the operations were not performed under his immediate inspection; they were executed by two observers, previously instructed at Madras, and dispatched with written directions,

* P. 322. The seconds pendulum at Port Jackson being 1·0056969, that at Paris is found equal to 1·00704998: hence the latter being 39·12929, the former will be 39·07682.

in

in the charge of a military party, to an island, Gaunsau Lout, off the coast of Sumatra, in lat. 0° 1′ 49″ north. During the whole time of the experiment the weather seems to have been very unfavourable to great accuracy in such delicate operations. Dropping useless figures, the length of the pendulum, as finally computed by Captain Goldingham, is 39·02126 at 70° Fahr., at which temperature the pendulum at London is 39·14243; wherefore at 62° Fahr., when the London pendulum is 39·13929, the pendulum at the equator must be 39·01812 or ·00598 longer than at Maranham $2\frac{1}{2}°$ south of the equator and about 141° of longitude westward; and ·00332 longer than at Rawak, on the equator, and 35° eastward. Captain Goldingham makes a small correction of his former determination of the pendulum at Madras, of no great moment; he then deduces the ellipticity by combining the corrected length with the pendulum at London; and, of this calculation, he remarks in a note, that the pendulum at the equator by computation is different from the measured one, ·00497740, which long number we may safely shorten to ·005 of an inch.

What, then, is the conclusion we are to draw from the whole of this discussion? The number of stations at which experiments have been made is now 39; and, of these, 28 concur in giving the same figure to the earth with very small discrepancies; but if we take the whole indiscriminately, and make certain combinations of them, we may obtain any ellipticity we choose. By what rule or principle are we to group the experiments so as to obtain some consistent knowledge of the figure of the earth and of the distribution of gravity on its surface? Is it to be admitted as a thing sufficiently proved, that gravity may vary at different points of the equator, or of the same parallel, to the extent of nearly $\frac{1}{20}$ of the whole increase from the equator to the pole? The experiments that may be alleged in support of this, are now formidable in number; but they rest on the authority of comparatively few observers, not always placed, perhaps, in such circumstances as to ensure the utmost precision. Shall we not, therefore, suspend our judgement and wait the award of future experiments, but of experiments to be so conducted that it shall be impossible to entertain a doubt of the correctness of the results? In the mean time, when any perplexing inconsistencies arise on comparing the pendulum experiments, instead of contending for this ellipticity or that ellipticity, we may coolly remark with Captain Goldingham, that the computed quantity is different from the measured one, ·003 or ·004 or ·005 of an inch!

J. Ivory.

LV. *On the mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the resulting Compound. By Mr.* Henry Hennell, *Chemical Operator at Apothecaries' Hall.**

THE following experiments were originally undertaken with the view of ascertaining the nature of that singular product of the distillation of sulphuric acid and alcohol, which has long been known in the pharmaceutical laboratory under the name of oil of wine, and which has generally been regarded by chemists as a modification of sulphuric æther. The results however of my inquiries have led me to very different conclusions, and induce me to regard it as a hitherto undescribed compound of sulphuric acid and carbon and hydrogen; the latter elements existing in the same proportions as in olefiant gas, and exerting a peculiar saturating power in respect to the acid. I have also ascertained that hydrocarbon, with an additional proportion of sulphuric acid, affords a compound which is capable of uniting with salifiable bases, and of forming a distinct series of products.

Of Oil of Wine.

As I originally considered the elements of oil of wine to be the same as those of æther, I endeavoured to ascertain their relative proportions by passing its vapour over red hot oxide of copper in a glass tube, in the apparatus contrived for such decompositions by Dr. Prout and Mr. Cooper, (Trans. Soc. Arts, xli. p. 56). In these experiments I always obtained, along with the other products, a considerable proportion of sulphurous acid, and afterwards upon washing the contents of the tube with water upon a filter, it was of a blueish tint, and held sulphate of copper in solution; a result which I could not readily account for, as every precaution had been taken to free the oil of wine which I used from all adhering sulphurous and sulphuric acid.

I now added a few drops of the same oil of wine to a solution of muriate of baryta and gently heated the mixture, when not the slightest cloudiness was produced, although litmus paper indicated the existence of a free acid; but upon evaporating the mixture a precipitate fell, when it became concentrated, and on boiling it to dryness, a considerable quantity of sulphate of baryta was found in the residue: it became evident therefore that the sulphuric acid was in some state of combination which prevented its usual action upon tests, or

* From the Philosophical Transactions, 1826, Part III.

that

that its elements were in some peculiar state of arrangement in the oil of wine.

To determine the quantity of sulphuric acid thus elicited, I boiled 200 grains of very carefully prepared oil of wine, free from all trace of acid, with a solution of caustic potassa to dryness; the residue was heated red hot and dissolved in water, the excess of potash being slightly supersaturated with dilute nitric acid; muriate of baryta was then added as long as it formed a precipitate, and 218·3 grains of sulphate of baryta were thus obtained. A repetition of this experiment gave the same results; so that we may conclude upon the presence of 74 grains of sulphuric acid in 200 of oil of wine.

In resuming the analysis of oil of wine by ignited oxide of copper, I found it necessary to mix it perfectly with the greater part of the oxide employed, otherwise, as in the first experiment, sulphurous acid was formed in consequence of the perfect reduction of a portion of oxide, and the action of the metal thus reduced upon the sulphuric acid. With this precaution several experiments were performed, the results of which were very uniform, and as follows: 2·08 grains of oil of wine carefully freed from all adhering moisture by quicklime, were properly mixed with 200 grains of oxide of copper, and subjected with due precautions to a red heat, in the apparatus formerly adverted to: the products were 8·8 cubic inches of carbonic acid gas, and 1·54 grains of water, and these are equivalent to 0·171 of a grain of hydrogen, and 1·118 grains of carbon: 100 grains therefore of oil of wine would afford

	Hydrogen . . .	8·30
	Carbon	53·70
		62·

and the deficiency of 38 grains must be referred to sulphuric acid, a conclusion which is verified by the former experiment with solution of potassa, in which the proportion of that acid is shown to be 37 per cent. We may I think therefore conclude the above estimate to be near the truth, and the results, as respects the carbon and hydrogen, approximate nearly to the proportional quantities 6 and 1.

From the above experiment, however, we can only infer the composition of the hydrocarbon, which is combined with and neutralizes the sulphuric acid; for in all the specimens of oil of wine which I have examined, I have found a variable quantity of hydrocarbon held in solution, part of which spontaneously separates in a crystalline form when it has been kept for some time, or when exposed to cold, but the whole of which I have not yet devised any means of separating; this dissolved

 hydro-

hydro-carbon, however, as future experiments will show, appears to be composed of single proportionals of its elements; we must therefore have recourse to other experiments to determine the actual weight of hydrocarbon in its neutral or atomic compound with sulphuric acid.

Having thus far made out the composition of oil of wine, I examined more carefully what had taken place during its action upon heated solutions of muriate of baryta and potash; in which case, as I have already observed, an acid had been formed not capable of precipitating baryta.

200 grains of oil of wine were placed in a flask with 5 or 6 ounces of water, and the flask set in a vessel of boiling water for an hour; precipitated carbonate of baryta was then added, and immediately dissolved with effervescence; about 90 grains of carbonate were required to neutralize the acid formed: the solution filtered and set to evaporate soon became acid, and sulphate of baryta precipitated. 200 grains more of oil of wine were treated in the same way, but instead of evaporating the baryta solution, it was precipitated by carbonate of potash; the potash solution evaporated at a temperature of 150° until it crystallized, remained perfectly neutral; the crystals were thin plates, not unlike chlorate of potash, greasy to the touch, very soluble in water and alcohol, burning when heated with a flame like that of æther, and leaving an acid sulphate of potash. A few grains of these crystals were heated in a tube, when they fused, swelled up, and gave off a dense white vapour, which condensed into an oil-like fluid, smelling strongly of sulphurous acid; the residuary salt was an acid sulphate of potash.

The following experiments were now undertaken with a view of more accurately determining the composition of this crystalline salt.

20 grains of the crystals being heated to redness, left 10·56 grains of sulphate of potassa, equal to 4·8 sulphuric acid, 5·76 potash. 20 grains were dissolved in a solution of caustic potash, boiled to dryness, heated red hot, when cold dissolved in distilled water, the excess of potash saturated by nitric acid, and the solution added to one of muriate of baryta; 28 grains of sulphate of baryta were obtained, very nearly equal to 9·6 sulphuric acid; the salt therefore contained twice the quantity of sulphuric acid required to form a neutral sulphate with the potash, or two proportionals.

In order to ascertain the proportions of the remaining elements of the salt, 5 grains were heated with oxide of copper, 5·5 cubic inches of carbonic acid gas, and 1·4 grains of water were collected; several of these experiments were made with

similar

similar results; 20 grains of the salt had been found to contain 5·76 potash,
9·60 sulphuric acid.

5 grains therefore must have contained	1·44 potash.
	2·40 sulphuric acid.
5·5 cubic inches carbonic acid gas contain	0·699 carbon.
The water obtained was	1·40.
	5·939 grains.

making an excess over the 5 grains employed of 939 of a grain. If this excess be oxygen furnished to hydrogen to form part of the water obtained, and such a view is confirmed by the loss of weight of the tube and its contents after the operation, it will give of hydrogen 0·1174 of a grain, and of water so formed 1·05 grains; this deducted from the whole quantity of water obtained, leaves 0·35 of a grain water of crystallization; 100 grains would therefore be composed of

Potash	28·84
Sulphuric acid . . .	48·84
Carbon	13·98
Hydrogen	2·34
Water	7·

These numbers indicate nearly one proportional of potash, two of sulphuric acid, four of carbon, and four of hydrogen; and it would appear that in these salts the four proportionals of carbon, with the four of hydrogen, are saturating one of sulphuric acid. I am not able at present to account for the difference between the quantity of water and a proportional number, every precaution having been taken in these experiments to insure accuracy. Several attempts were made to procure an anhydrous salt, but without success, in consequence of the facility with which these acids and other compounds decompose.

The resemblance of these salts to the sulphovinates, induced me to suppose they might be similar in composition. I therefore prepared some sulphovinate of potash. Its crystalline form was the same as that of the salt obtained from oil of wine, and upon examination it proved in all respects similar.

While preparing some of the sulphovinates, I was struck with the very great change produced in sulphuric acid by mere mixture with alcohol.

440 grains of sulphuric acid were mixed with an equal weight of alcohol of specific gravity ·820; the mixture when cold was diluted with water and saturated by carbonate of soda, partially dried, of which it required for saturation 398 grains,

grains, while 440 grains of sulphuric acid not mixed with alcohol saturated 555 grains of the same carbonate of soda, so that 2-7ths of the acid had been saturated by the alcohol.

440 grains of sulphuric acid mixed with its own weight of alcohol, as before, and then poured into a solution of acetate of lead, 542 grains of sulphate were precipitated. The same quantity of sulphuric acid unchanged by alcohol gave 1313 grains of sulphate of lead; thus 4-7ths of the sulphuric acid had lost its power of precipitating oxide of lead from its solutions; it had in fact been converted into sulphovinic acid.

M. Vogel, who has particularly described some of these salts, and I believe also M. Gay-Lussac, have supposed that this loss of saturating power arises from the formation of hyposulphuric acid, and that the hyposulphates, and sulphovinates, only differ in the latter containing some æthereal oil, which in some way acts the part of water of crystallization. It is evident that the properties of oil of wine cannot be thus explained; and it appears to me more probable that the power of combination which hydrocarbon is shown to be possessed of in oil of wine, is effective in neutralizing half the acid of the salts formed from it, as before described.

It only now remains to examine the hydrocarbon in the states in which it has been obtained separate from its combinations.

When oil of wine is heated in a solution of potash, or if heated in water merely, the excess of hydrocarbon above that necessary to constitute the acid, forming the salts I have described, is liberated in the form of an oil, not unlike in appearance castor oil, having but little fluidity when cold, and in some cases partially crystallizing. When gently heated it is beautifully bright, and of an amber colour; the vapour has an agreeable pungent and aromatic smell; it evaporates at a temperature a little above that of boiling water; burns with a brilliant flame, throwing off some carbon; its specific gravity is about 9, water being 10; it is insoluble in water, very soluble in æther, somewhat less so in alcohol.

Several analytical experiments were made upon this substance with similar results. When decomposed by oxide of copper, 0·72 of a grain gave 4·85 cubic inches of carbonic acid gas and ·85 of a grain of water; 4·85 cubic inches of carbonic acid gas are equal to 0·6164 of a grain of carbon, and the 0·85 of a grain of water to 0·09444 of a grain of hydrogen; 100 parts should therefore be composed of

Carbon	85·61
Hydrogen . . .	13·116.

There is here some trifling loss; if that be supposed to be hydrogen,

hydrogen, this oily matter is precisely similar in the proportions of its elements to olefiant gas.

The crystals which spontaneously separate from oil of wine were next examined; they were prismatic, and resembled precisely in all their characters, except their solid form, the fluid substance just described. They fused at a temperature a little above that of boiling water. After purifying a portion by pressing them in blotting paper, to remove any adhering oil of wine, several experiments were made upon quantities of a grain each; 6·46 cubic inches of carbonic acid gas and 1·21 grains of water were obtained; the 6·46 cubic inches of carbonic acid equal ·82106 of a grain of carbon, and the 1·21 grains of water equal ·13444 of a grain of hydrogen.

The carbon and hydrogen are here very nearly in single proportionals, but there is great loss, I believe: this may be partly attributed to oil of wine still adhering, but of that I am not at present able to satisfy myself, my stock of crystals being exhausted.

On mentioning these results to Mr. Faraday, he gave me some sulphuric acid which had been exposed to olefiant gas, during some of his experiments on the products of the decomposition of oil by heat. It had absorbed about 80 times its volume of the gas, acquired a deep brown colour, and a smell resembling oil of wine. It was saturated by carbonate of potash carefully evaporated to dryness, and the dry mass digested in alcohol. A small quantity of a salt was obtained from the alcoholic solution having the crystalline form and general characters of the salts I have been describing.

Thus it would appear that hydrocarbon constituted of single proportionals, or 6 carbon and 1 hydrogen by weight, has the power of combining with sulphuric acid: and that whether it be evolved and then combined, as in the case of olefiant gas, or its elements separated from other compounds, as from alcohol, it forms precisely the same combination, sulphovinic acid. It further appears that oil of wine is a perfectly neutral compound of sulphuric acid and hydrocarbon, and that it is resolvable by various processes into sulphovinic acid, during which it loses hydrocarbon, and acquires a saturating power equivalent to only half the natural power of the sulphuric acid it contains. The remaining hydrocarbon enters with it into saline combinations, and is there equivalent in saturating power to the quantity of base taken up. The proportions of hydrocarbon which in this way replace bases being four proportionals, or 24 carbon, 4 hydrogen.

LVI. *Che-*

LVI. *Chemical Researches on Starch, and the different amylaceous Substances of Commerce.* By M. J. B. Caventou.

[Concluded from p. 293.]

Reflections on the true and original nature of Sago and of Tapioca.

Do sago and tapioca, in the state in which they have been submitted to our examination, exist in the plants which produce them ?, or in other terms, Do not these vegetable products, after having been extracted from their respective plants, undergo from the natives some manipulations insignificant in appearance, yet nevertheless capable of modifying their chemical nature? If we rely on the reports made by naturalists on the subject of the extraction of these two amylaceous substances, recollecting also what precedes, we shall not, I hope, encounter great difficulties in resolving the question. In fact, sago is white when it is extracted from the medullary part of the palm which contains it; it suffices to crush it and to dilute it in water, then to pass through a sieve the kind of amylaceous milk which results from it, and to let the liquid settle; the sago is precipitated under the form of a white powder, and very fine, which is easily collected to be dried. It is nearly the same with tapioca; but according to the simple detail of this manipulation, is it not evident that these two bodies are insoluble in cold water, and that they do not act in this liquid, as we have remarked in regard to the same principles as they exist in commerce?

But if we follow up to their completion the methods of extraction and preparation of these substances, we find that tapioca while yet damp is slightly heated in wide basins, in order to *dry* it and make it fit for *granulation*; that sago is submitted to a similar operation, and that the natives carry the desiccation of this latter even to the first degree of torrefaction, in order to give it the reddish tinge which characterizes it. Is then any thing more needed to modify the chemical nature of these amylaceous feculæ ?

Nevertheless it appears that the state of these substances in commerce is not always regular, which, indeed, need not be wondered at, in consequence of the irregularity of the modes of desiccation to which they are submitted in the country: we may easily conceive that the solubility may become complete or partial, accordingly as the temperature employed in the desiccation has been more or less raised, prolonged, and applied with less or more uniformity to the mass of these substances.

Thus

Thus I received some sago which had only a very slight degree of solubility in cold water, and some which had none at all. M. Boutron gave me also at the same time three samples of tapioca; one came from the islands, and he supposed the other two to be factitious; that is to say, to have been fabricated in France. The tapioca of the islands was acted on by cold water like that which had been the subject of my experiments: as to the factitious specimens, they appeared to be composed of two distinct substances; the one was in round, transparent, and pretty regular grains; the other, in very irregular, larger, opaque grains, presented all the counterfeit appearance of the tapioca of the isles.

These two substances have been isolated and put in contact with cold water, for comparison with the tapioca of the isles. The three liquors acted in the following manner: with iodine the maceratum of the transparent grains took a slight blueish green tint, that of the opaque grains did not change, whilst the maceratum of the natural tapioca took a beautiful blue colour. We must not, indeed, attribute more importance to these results than they merit; for there is no actual proof that it was factitious tapioca with which I had to do: besides, it would not be surprising to find a tapioca very pure, quite American, and which would be completely insoluble in cold water.

If, as every thing leads us to believe, spurious tapioca is manufactured in France with the fecula of potatoes, nothing appears more difficult to me than to distinguish it *chemically* from the genuine which comes to us from the West Indies, especially if the sort of baking which the moist fecula is made to undergo for granulation is well managed. Yet the very decided swelling which genuine tapioca undergoes in cold water, and which in my opinion the fecula of our own country cannot so well exhibit, might perhaps afford some indications of spuriousness. Be that as it may, I see no harm in such a substitution, when it is publicly excited by the love of our country, and not by a vile and interested cupidity.

I diluted in cold water some recent *empois* of starch: after the starch not decomposed or modified had become precipitated by rest, the liquid was decanted and filtered, and was perfectly transparent: tried by the following reagents, it was rendered slightly turbid by the sub-acetate of lead, gave an abundant precipitate with gall-nuts, and took a beautiful blue colour with iodine. The aqueous maceratum of tapioca from the islands, and the cooled and filtered decoction of the same substance, are acted on in like manner by the same reagents; the turbidity produced by the sub-acetate of lead is only less sensible in the

maceratum than in the decoction; but, as we have before said, these results may vary according to the circumstances in which the liquors of trial have been made; and it appears to us very easy to explain the cause.

Thus, from all these comparisons between the fecula of potatoe, sago, and tapioca, we may then conclude; 1st, that if these two latter substances differ from the first by their solubility in cold water, they are not the less of a similar nature, and that the difference of their state in commerce is owing to the mode of extraction and preparation employed, with regard to them, in the countries where they grow.

2dly, That it is easy to give a similar property to the fecula of potatoes, in like manner as is practised for the manufacture of the *poulinta* of M. Cadet of Vaux, and very likely for the French tapioca.

3dly, lastly, That in these operations, one of the most decisive properties of the amylaceous fecula is destroyed, whatever be the vegetable from which it was extracted; which, in my opinion, should cause it to be identified with the new vegetable principle designated by the name of *amidine* by M. de Saussure.

Before I conclude, I must state that I do not comprehend under a *perfect identity* all these feculæ extracted from such different vegetables. The last observations of M. Planche on the fecula of the black radish, and the very interesting ones of M. Payen relative to the differences these feculæ present in their specific gravity, would be quite sufficient to destroy such an assertion:—my only object was to prove their *great chemical analogy*, by the comparative progression of the phænomena which they present in the same circumstances, in the same manner as I could cite examples with regard to some other animal and vegetable matters.

Observations on the new manner in which M. Raspail *considers the Fecula of Potatoe.*

If the observations which M. Raspail has just published on the subject of the fecula of potatoes are correct, the chemist will henceforward no longer be permitted to proceed without a microscope. For a long time starch has been regarded as one of the best characterized immediate principles of vegetables; the most celebrated chemists have never had but one opinion in this respect;—besides, do not the most authentic and the best verified facts prove it in an evident manner? And, notwithstanding, these chemists would have been deceived. Their experiments would have been insufficient to

rescue

rescue them from an error which the microscope alone could disclose to them. I own, that, in the physical sciences, there can only be, on the part of those who cultivate them, conditional or relative belief; the progress of the human mind, more and more enlightened from time to time, imperiously requires it. But I also feel regret in abandoning an opinion which had become a sort of habit, since it appeared so well confirmed by time and experience: I am also no longer surprised, that chemists, very commendable in other respects, should have carried to their graves devotedness to, and conviction of the doctrine of Stahl, their master.

Be that as it may, if we are not convinced that the new theory by which M. Raspail explains the chemical phænomena exhibited by starch, is exact and well founded enough in order to admit it, at least it will have the merit of proving to chemists that the application of the microscope in their habitual labours may sometimes make them witnesses of curious phænomena.

I shall now examine the principal assertions given by this author in his Memoir: but first I think I should note his most important conclusions.

1st, Fecula is composed of vegetable organs in the form of globules.

2dly, Each grain of fecula is formed, 1st, of a smooth integument, which cannot be attacked by water and acids at the ordinary temperature, susceptible of being coloured a long time by iodine; 2dly, of a soluble substance, which evaporation deprives of the faculty of becoming blue by iodine, and which possesses all the qualities of gum.

3dly, Consequently, the gums which flow from vegetables are nothing but this soluble substance of the fecula, which has lost from being in the open air the faculty of becoming blue.

Lastly, The faculty of becoming blue by iodine is owing to a volatile substance.

I have witnessed, through the kindness of Mr. Edwards, who had a microscope at his command, that the grains of the fecula of potatoes (it is with this substance that the author has always made his experiments) have a globular form, the diameter of which varies without end: but nothing proved to me that these grains contained a gummy substance. M. Raspail submitted these grains of fecula to the action of tincture of iodine, upon the object-slider of the microscope: he saw them acquire a blue colour, without losing their form thereby; and then, having submitted them to the action of potassa and ammonia, he saw them lose their colour immediately, without undergoing the least alteration in their form and appearance:

 from

from thence the author concludes that the *iodide of starch* does not exist, and consequently that there is no chemical action between the iodine and the starch, and that it is only a simple superposition of the molecules of the iodine on the surface of the grains of fecula.

This experiment proves, in my opinion, that the grains of fecula are not endowed with a porosity great enough to permit the absorption of iodine into the interior of their substance: and that as the chemical action is very feeble between these two bodies, there is no disaggregation of the amylaceous molecule, which then preserves its primitive form; but it is no reason for not admitting a combination, superficial as it may be. The animal and vegetable tissues submitted to the processes of dyeing, do they lose their pristine form by the fixing of the colours?. And notwithstanding I think it will not be said that there is no combination.

From the circumstance that the grains of fecula are all formed and free in the cells of the vegetable; of their rounded and smooth form; of their unalterability in cold water, their colouration by iodine and their discolouration by alkali,—M. Raspail concludes that fecula is not an immediate principle of vegetables; and a series of experiments which he has made seems to prove to him that the grains of fecula are organs formed of a smooth integument, not to be attacked by acids at an ordinary temperature, and of a substance therein inclosed, which he thinks of the same nature as gum. We are about to give in succession these experiments, and to discuss the consequences of them.

This author exposed some fecula to heat, in such a manner as to carbonize the upper layer; he then made haste to project some parcels of the intermediate layer of fecula on the object-slider, on the centre of which he put a drop of diluted alcohol. All at once he saw currents become established, grains of fecula attract and repel one another with the rapidity of lightning; and he *perceived*, he says, certain gummy traces which spread themselves slowly in the liquid, as if even the much-diluted alcohol could thus permit the gum to diffuse itself. The author adds, that if the fecula is coloured before it is exposed to the action of fire, by the microscope we see the liquor come out of it colourless; with pure water, the experiment is not so decisive, because this liquid too quickly dissolves the gummy part: nor are we constantly successful with the diluted alcohol; and we must always, according to the author, repeat the experiment often in order to see the liquid portion come out of its envelope; but we always find on the object-slider a mass of insoluble integuments and a gummy substance which

which may be separated from these by dissolving it in a drop of water. It is easy to judge from the inconstancy of these results, that it is very difficult to observe well with a microscope.

Without admitting the existence of the integuments and of the gummy part known here by very uncertain experiments, is it not more simple and more easy to have recourse to the chemical knowledge we possess to explain these phænomena?

It is very well known that starch changes its nature by the direct action of heat or that of hot water; it is not astonishing then that, under the three circumstances in which M. Raspail microscopically examined this substance, he saw water disengaged from it: it must doubtless be this result which effected a change in the molecular form.

It is notwithstanding according to the result of the three preceding experiments that the author further strengthens himself in his idea of the composition of the grains of fecula.

M. Raspail submitted some fecula to the action of boiling water, and he saw a part of the preceding phænomena again produced; that is to say, torn integuments swim in the liquid, separated from their gummy part which he supposes to be contained there, and which in his opinion had become dissolved in the water. Thus the *empois*, instead of being a combination of water, of amidine and of starch, would be a solution of gum extracted by the water from a certain number of grains, torn integuments coming from these last, besides some grains of untouched fecula, but diluted with hot water. According to the author, it would be easy to separate the integuments of the soluble matter; it would suffice to dilute the *empois* with a great deal of cold water, the gum would pass dissolved, and the integuments would remain on the filtre.

Respecting this experiment, there is a difficulty which stopped M. Raspail a long time, and which I think he has not yet removed: if the gum contained in the grains of fecula does not differ from other known gums, as the author says, it ought not to become blue by iodine; which is what M. Raspail first believed, and having already ascertained the insolubility of the integuments in water, he had in the afore-cited experiment, a means of proving the composition of the grains of fecula. In fact, filtration gave him a gummy liquor on the one hand, and on the other integuments on the filtre. The former should not undergo any colouration by iodine, and the latter ought directly to take a beautiful blue colour: but the result did not take place thus; the integuments and the gummy liquor equally became blue, which exceedingly embarrasses M. Raspail; he repeated and varied this experiment in every

manner,

manner, and he always obtained the same result. Such a fact gave too rude a blow to the principal idea of the author for him not to verify it in every way: he had also recourse to his microscope, which enabled him to perceive in the gummy liquid a great number of integuments; and as these are the cause of the colouration, the fact, contradictory in appearance, was found quite natural; and it even offered to the author an additional motive to persist in his view. We then find M. Raspail convinced that the integumentary part alone becomes blue by iodine, and that if the gummy part partakes of this property, it is by means of this last drawn through the pores of the filter.

With regard to this, I shall observe, that if things took place thus, the blue colouring by iodine ought to decrease in the liquor with the number of the integuments.

And yet M. Raspail says: that since the microscope only indicated to him at the most the presence of one integument in a square inch, he had only to pour some iodine in his liquid, and it became quite as blue as with the fecula itself: it is necessary then to conclude from it, as appears to me, that there is in *empois* a substance which becomes blue by iodine, independently of the integuments.

The author had this thought, for he could not reject the facts of which he was himself a witness. Moreover, a few lines lower, he admits that the gummy part may also become blue by iodine when it is in solution, and he attributes it to the formation of membranes in the liquor, which disappear as the blue colour is effaced.

What is to be concluded from this fact, says M. Raspail? That the fecula does not become coloured by iodine except when it is in a membranous form. It is on this account that the integuments always remain coloured.

Thus, according to M. Raspail himself, here is the gummy part which approaches singularly, by its nature, to the integuments, since this is as suceptible as them of taking, even in a state of solution, a membranous form, which then permits it to take a blue colour with iodine. But since, in spite of the most careful filtrations of these gummy liquors, the microscope has always indicated the presence of some integuments, and since these always remain coloured, how is it that these liquors lose, at the end of twelve or fifteen hours contact with the air, their blue colour, which they regain by the addition of a new dose of iodine? The integuments then would not always remain coloured, in the same manner as the supposed gummy part. It appears to me very difficult to reconcile all these facts in the manner of M. Raspail.

To

To proceed: "The soluble substance," says this author, "not only loses in the open air the blue colour which the iodine communicated to it, but moreover the action of heat takes away its property of again becoming coloured. It is known that the syrup of fecula is prepared by the apothecaries when iodine no longer colours the amylaceous substance. This phænomenon has been attributed," he says, "to a metamorphosis produced by a long ebullition: we were far from adopting explanations of this kind so much used nevertheless in vegetable chemistry, and the following observation is sufficient to remove every idea of metamorphosis."

See, now, the capital observation which prevents M. Raspail's admitting the modifications so much used in vegetable chemistry.

"Evaporate," he says, "the soluble substance of the fecula obtained as far as possible in the highest state of purity, and let it be evaporated in layers not very thick; a substance will be obtained entirely similar to gum in its physical characters, and its no longer colouring iodine, either in a solid state, or dissolved in water. The colouring of the fecula is then certainly owing only to a foreign and volatile substance, which evaporation causes to disappear."—(Page 395.)

What astonishes most in an assertion so new, and I would almost say so unexpected, according to the preceding explanations, is the facility with which M. Raspail admits the existence of a volatile substance which he neither saw nor obtained: just now, the colouration was inherent in the membranous form of the integuments and to that analogue which can affect the gummy part in certain circumstances; and now, that the microscope no longer indicates any trace of membranes or of integuments, M. Raspail borrows from his imagination a volatile substance, by the help of which he escapes from the difficulty;—this seems very convenient. Thus the integuments, like the gummy part dissolved in water, now no longer become coloured by iodine but by the help of a volatile substance; but then what becomes of the theory of the colouration and the decolouration of the filtered liquor, and proceeding from the *empois* diluted in cold water? This colouration then is no longer owing to the integuments that have passed through the pores of the filter, but to the gummy part, which can affect in this case a membranous form. It seems to me again very difficult to reconcile all these different explanations of one and the same fact, of which notwithstanding a simple chemical alteration gives in my opinion so satisfying an account.

I shall follow up this examination no further: the various citations which I have just made from the Memoir of M. Raspail will

will suffice, I think, for appretiating whether the new theory which he proposes on the subject of starch, is as well founded as he wishes us to believe.

As for me, I am convinced of the contrary, and cannot think, that, in all the circumstances cited by this naturalist, a view by the microscope can take the place of a chemical observation. If this instrument should one day be of known utility in chemistry, this will without doubt be one additional means of investigation, and of which we ought to be glad: but I may be permitted to doubt it until more precise observations, and which agree more with facts, shall have convinced me.

Besides, it is to be remarked, that in the whole course of his Memoir, M. Raspail does not indicate any means fit to isolate effectually the gum which he says is contained in the integuments that constitute fecula. The most powerful analytic means, and by the help of which he establishes the greater part of his deductions, is always to be found in his microscope; and when the microscopic observation does not agree with the chemical observation, you have seen by what means M. Raspail reconciles things. He supposes the existence of a volatile substance. You will agree without doubt that such a manner of acting and reasoning swerves too much from that now followed in the study of the exact sciences to be admitted. If this were not the case, we should run too much risk of going back again towards the time when flourished *phlogiston* and *acidum pingue*, which would again return, without doubt, under forms and names more appropriate to our epoch.

Thus, until M. Raspail shall have proved it by chemical experiments more decisive than those on which he rests in his Memoir, I cannot bring myself to believe, 1st, in the composition of fecula as formed of integuments and gum; 2dly, in the colouration of this principle by iodine, in the sense that this phænomenon is only a superposition of iodine upon the grain of the fecula, and not a combination; and, 3dly, that the cause of this colouration is only owing to a volatile principle.

Nevertheless I admit with the author the globular configuration of the molecules of fecula; but I believe them homogeneous, and not of two different natures.

I am unwilling to terminate this Memoir without offering a just homage to M. Raspail. If I have controverted the view which he takes, I do not the less render justice to the distinguished talents of which he has given proof in his Memoir. I am persuaded that he is more than capable of completely resolving the question, equally new and ingenious, which he

he has raised. I sincerely desire that he may attain it, for the interest of truth and also for my instruction: this avowal will suffice without doubt to make this estimable author appretiate the motives which have induced me to publish these reflections.

P.S. I have just become acquainted with a work entitled *Memoir on the Structure of the Potatoe*, by M. A. Villars, Dean of the Faculty of Medicine in Strasbourg, correspondent of the Institute, &c.

This work, inserted in volume xlii. of the *General Journal of Medicine*, at present edited by Dr. Sedillot, containing some experiments and views calculated to throw some light on the question which I have treated with relation to the homogeneity of the fecula, as well as on the priority of microscopic observations of this nature, I have thought it useful to extract from it the following passages at the conclusion of my Memoir.

(Paragraph 19.) "The flour of potatoe is formed of ovoid globules from $\frac{1}{100}$ to $\frac{1}{50}$ of a line in diameter, and about a third more in length. I had observed them at Grenoble, noted and drawn them in 1802, perfectly conformable to what I observed at Strasbourg in 1810. I again took to this pursuit with more details and variations *with new microscopes*. These globules are smooth, brilliant and milky, like globules of mercury.

"§ 20. The globules of the fecula of potatoe crushed on the glass as much as possible with a steel blade, are smaller by half or two thirds, and are rather square or irregular, but always smooth.

"§ 21. The same globules cooked in the potatoe are about a third larger, more rounded, and less even, not shining, but as if cracked or split on their surface, being seen of the same size at a hundred diameters.

"§ 22. The same globules seen through a very thin slice of about $\frac{1}{10}$th of a line thick, taken in a frozen potatoe, appeared to me less by one-half, surrounded with water and as it were deliquescent. I could then see, with the same lens, the tissue of the potatoe or the fibrin which contains the globules; they are disseminated through it by groups in contact but not adherent to the meshes of the tissue.

"§ 23. I dried on glass and on plates the fecula of potatoe, in a stove and in a sand-bath, until the globules began to redden. They lost a little of their size and a little of their brightness. The light then penetrates them only towards the centre, whilst the circumference appears opaque, as if they were bubbles of air seen by the microscope. In this state, I crushed them

them as well as I could between two pieces of polished glass; they always appeared globular, but less, and more greedy of water than before. I do not doubt but that these globules contain, like salts, a water of crystallization.

"I had to examine the fine flour of wheat in order to compare the molecules of it with those of the potatoe; they are smaller, and are rendered more irregular, by globules and by shapeless molecules in the wheat. I cut some transversal slices very thin of a grain of wheat; round and very uniform globules then appeared, but three times smaller than the globules of the potatoe; for they were only from $\frac{1}{250}$ to $\frac{4}{100}$ of a line in diameter. Neither hair-powder, nor starch in *empois*, seen through the microscope, essentially differs from flour.

"I do not presume that this diminutiveness is the only cause which renders flour fit for powder; but heat does not change it so easily as the fecula of potatoe: it contains less water; it is also less quick in losing and regaining it. Also potatoe-bread keeps fresh but two or three days, that of wheat from four to eight days; whilst rye bread keeps fresh at least from fifteen to twenty days, even a month, when the bran is left with it, at least, in part."

LVII. *On the Diamond Mines of Southern India.* *By* H. W. Voysey, *Esq.**

HAVING lately visited some of the principal diamond mines of Southern India, the few facts I have been able to collect respecting the geological relations of that gem, I take the liberty of laying before the Asiatic Society.

A knowledge of the matrix of the diamond has long been a desideratum in mineralogy. It has been hitherto supposed that this mineral was only found in alluvial soil; and a late writer infers from some circumstances attending a particular diamond, which had passed under his examination, that the matrix of this precious stone was neither a rock of igneous origin nor one of aqueous deposition †, "but that it probably originates like amber, from the consolidation of perhaps vegetable matter, which gradually acquires a crystalline form, by the influence of time, and the slow action of corpuscular forces."

This reasoning may apply with justice to the particular specimens which have fallen under the observations of Dr. Brewster, but as it is fully ascertained that diamonds have

* From the Bengal Asiatic Researches, vol. xv. p. 120.

† See Quarterly Journal of Science and Art, Oct. 1820.

for

for two centuries at least been found in *a rock*, generally supposed to owe its origin to deposition from water, the application will of course be limited to the case of diamonds found in alluvial soil.

A considerable range of mountains called the Nalla Malla* (Blue Mountains?) lies between the 77° and 80° of east longitude. Their highest points are situated between Cummum, in the Cuddapah district, and Amrabad, a town in the province of Hyderabad north of the Kistna, and vary in height from 2000 to 3500 feet above the level of the sea. The following barometrical † heights are taken from my own observations, the others are from trigonometrical calculations with which I have been favoured by Colonel Lambton.

Trigonometrical heights above the level of the sea.	Barometrical heights above the level of the sea.	
3086 feet	3060 feet	Durgapah-condah, a hill station of Colonel Lambton.
	1767	Pass between Cummum and Nandial.
	1563	Temple of Sri Sailam ‡.
	2520	Ruined Temples and Stone Tank S.E. of Sri Sailam, 5 miles.
3149	not visited	Byramcondah, hill station of Colonel Lambton.
3055	not visited	Cundah-Brahmeswar, hill station of Colonel Lambton.
	717	Town of Nandial.
	507	Town of Cuddapah.
	1000	Bed of the Kistna at Moorcondah.

The outline of these mountains is flat and rounded, very rarely peaked; and as they run N.E. and S.W. the ranges

* I have reason to believe that this name is merely local.

† The barometer is a late contrivance of Sir Harry Englefield. It is called the Box Barometer, and is refilled at every station with purified mercury. The cistern is of box-wood and open; with a gauge an exact inch in height, which is adjusted by a lens at the time of observation. It differs but in a trifling degree from other barometers with which it has been compared, and I believe that its horary variations are more uniform. From the close correspondence usually observed between the trigonometrical and barometrical heights at many of Colonel Lambton's stations, I think the maximum of error is not more than 50 feet.

‡ Pagoda of Perwuttum described by Col. Mackenzie in the Asiatic Researches, vol. v.

gradually diminish in height, until in the former direction they unite with the sandstone and clay slate mountains of the Godavery near Palúnshah. Their union is certainly not very distinct, but is sufficiently so to entitle them to be considered geologically as the same range. In a southern and S.W. direction, they probably extend considerably beyond the Pagoda of Tripati. The most southern point that has fallen under my observation is Naggery Nose, a well known sea-mark on the coast of Coromandel. Travellers to Hyderabad make a considerable detour for the purpose of crossing these mountains in their most accessible parts. Among the western passes on the Cuddapah road are those of Bakrapet and Moorcondah on the bank of the Kistna, and those of Nakrikul and Warripalli on the Ongole road are among the eastern. The breadth of the range varies, but never exceeds 50 miles.

The geological structure of these mountains it is difficult to understand, and it cannot be easily explained by either the Huttonian or Wernerian theories. The different rocks of which they are composed, being so mixed together without regard to order of position, each in its turn being uppermost, that it is not easy to give a name so definite as to apply in all places. I once thought the term "shistose formation" would be the most simple and untheoretical term; but as clay slate is probably the most prevalent rock, I have determined on giving that name to the whole, observing however that by "clay slate formation" I do not mean the Wernerian Thousheiffer, the fourth in order, of his enumeration of primary rocks, but merely a collection of rocks which I conceive to have been placed in their present situation at the same period of time.

The "clay slate formation" then of the Nalla Malla mountains consists of clay slate; of every variety of slaty lime-stone between pure lime-stone and pure slate; of quartz rock; of sandstone; of sand-stone breccia; of flinty slate; of hornstone slate, and of a lime-stone which I call tuffaceous for want of a better name, containing imbedded in it, rounded and angular masses of all these rocks. All these vary so much in their composition, and pass into each other by such insensible gradations, as well as abrupt transition, as to defy arrangement and render a particular description useless.

It is bounded on all sides by granite, which every where appears to pass under it and to form its basis.

Some parts detached from the main range, such as Naggerry Nose, Worramallipet and Nandigaon, a town on the Hyderabad frontier, with many others, have only the upper third of their

their summits of sandstone and quartz rock; the basis or remaining two-thirds being of granite *.

This range of mountains is intersected by the rivers Kistna and Pennar, and both appear to pass through gaps or fissures in it, which have been produced by some great convulsion, which at the same time that it formed the beds of these rivers gave passage to the accumulated waters of some vast lakes situated near the outlets.

The tortuous passage of the Kistna for upwards of seventy miles is bounded by lofty and precipitous banks, which in some places rise to 1000 feet above its bed: the opposite sides of the chasm corresponding in an exact manner. Ravines of this description are not unfrequent all over the range, and the exact correspondence of their opposite salient and re-entering angles, together with the abruptness of their origin, totally preclude the supposition of their being hollowed out by the action of running water.

Two of these remarkable chasms occur on the western road to the shrine of Maha Deo at Sri Sailam, and would be totally impassable to travellers, but for the more magnificent causeway and steps, which wind down the precipice.

The only rock of this formation in which the diamond is found is the sandstone breccia. I have as yet only visited the rock mines of Banganpalli, a village situated about twelve miles west of the town of Nandiala. The low range of hills in which these mines are situated appear distinct from the main range, but a junction of the north and south extremities may be traced with great facility.

The breccia is here found under a compact sandstone rock, differing in no respect from that which is found in other parts of the main range. It is composed of a beautiful mixture of red and yellow jasper, quartz, chalcedony and hornstone of various colours, cemented together by a quartz paste. It passes into a puddingstone composed of rounded pebbles of quartz hornstone, &c. cemented by an argillo-calcareous earth, of a loose friable texture, in which the diamonds are most frequently found.

Some writers have miscalled this rock amygdaloid or

* I have reason to believe, partly from personal observation, and from specimens obtained from other sources, that the basis of the whole peninsula is of granite.

I have traced it along the coast of Coromandel lying under *laterite* (Buchanan's name for the iron-clay of Jamieson) from Pondicherry to Masulipatam.

From Rajahmundry to Nandair in the bed of the Godavery.

And I have specimens from the base of the Seetabaldi hills, Nagpoor. From Travancore, Tinnevelly, Salem and Bellary.

wacken,

wacken, and have described these mines as being situated on conical summits of that rock. The truth is that the conical summits are artificial, and owe their origin to the sifting of the pounded breccia and puddingstone, for the purpose of separating the larger stones, preparatory to their being wetted and examined. The hill itself is quite flat, and not a single conical elevation can be seen throughout its entire extent. In my journey from Nandiala on horseback, a view of the range for an extent of twenty miles N. and S. was constantly before me, and in no instance did I observe a deviation from the continued flatness.

I regret that for many years previous to my visit to these mines, no fresh excavations had been made, so that I had no opportunity of ascertaining the mode in which the miners get at the breccia. I saw many holes under large blocks of sandstone, of about five feet average depth, most of them blocked up by rubbish. I was told that at that depth the diamond-bed was found.

The miners are now content to sift and examine the old rubbish of the mines, and they are the more bent on doing this, from an opinion which prevails among them, and which is also common to the searchers for diamonds in Hindustan and to those on the banks of the Kistna, Parteala, Malavilly, &c. viz. that the diamond is always growing, and that the chips and small pieces rejected by former searchers, actually increase in size, and in process of time become large diamonds. I saw at the time of my visit in January 1821, about a dozen parties at work, each composed of seven or eight people. Each party was on the top of one of the conical eminences, and actively employed in sifting and separating the dust from the larger stones: these were then laid in small heaps, spread out on a level surface, wetted, and examined, when the sun was not more than 45 degrees above the horizon. A party of boys was engaged in collecting and pounding scattered pieces of breccia. All the labourers were *Dhérs* or outcasts, and under no controul or inspection. The misery of their appearance did not give favourable ideas of the productiveness of their labour.

The sandstone breccia is frequently seen in all parts of these mountains at various depths from the surface. In one instance I observed at a depth of 50 feet, the upper strata being sandstone, clayslate and slaty limestone. The stratification of the whole face of the rock is here remarkably distinct, and may be traced through a semicircular area of 400 yards diameter. The stratum of breccia is two feet in thickness, and immediately above it lies a stratum of puddingstone composed of

quartz

quartz and hornstone pebbles, cemented by calcareous clay and grains of sand. It is very likely that this stratum would be found productive in diamonds; and I have no doubt, that those found at present in the bed of the Kistna, have been washed down from these their native beds, during the rainy season*. In the alluvial soil of the plains at the base of this range of mountains, and particularly on or near the banks of the rivers Kistna and Pennar, are situated the mines which have produced the largest diamonds in the world. Among them are the famous mines of Golcondah, so called from their being situated in the dominions of the sovereigns of Golcondah, although they are far distant from the hill fort of that name,—from which the province and Cooteb Shahi dynasty took their title. They were once very numerous (at least twenty in number), and Gani Parteala, situated about three miles from the left bank of the Kistna, was the most famous. They are now, with the exception of two or three, quite deserted, and the names of several of those mentioned by Tavernier are forgotten. In none have fresh excavations been dug for many years; although much ground remains unopened, and many spots might be pointed out for new and productive mines.

Even at Gani Parteala the search is confined to the rubbish of the old mines: at Atcúr, Chintapalli, Barthenypard and at Oustapalli, all situated within two or three miles of each other, there are no labourers.

The plain in which these villages are situated is bounded on all sides by granitic rocks, which also form its basis. The average depth of the alluvial soil is about twenty feet†. Its upper portion is composed of that peculiar black earth which is called by Europeans "black cotton soil ‡," and is identical with that found on the banks of the Kistna in other parts of its course; on the banks of the Godavery; of the Manjera; Baen-Gunga and in the plain of Nandiala, arising from the decomposition of the basaltic trap rocks, in which all these rivers or their tributary streams take their rise. Beneath this

* Diamonds are found in the bed of the Godavery near Buddrachellum. The nullahs and small rivers which run into it near that place, have their origin in a rock formation exactly similar with those above described. I think it very probable that the diamond mines of Sembhelpoor, of Pannah, and even of Bijapúr are situated near similar rocks.

† The greatest extent of the alluvium from the river's bank is about six miles, and the change to the red and grey soil from the decomposition of the granitic rocks is very distinct.

‡ This soil is easily fusible before the blow-pipe: in 1820 I exposed it in a covered crucible to little more than a red heat, and it was converted into a light porous lava; before the blow-pipe it forms a vitreous globule.

of

upper stratum, it is mixed with masses and rounded pebbles of sandstone, quartz rock, jasper, flinty slate, granite and large amorphous masses of a calcareous conglomerate, bearing no mark of attrition from the action of running water. In this stratum the diamond and other precious stones are found. The excavations are of various size, but from 15 to 20 feet deep.

The labourers are a little more under controul than at Banganpali, and they pay a trifling duty to the Nizam's agent stationed in the village. The mode of search is precisely the same as that above described.

The mines of Ovatampalli and of Canparti on the right and left banks of the Pennar near Cuddapet, are in an alluvial soil of nearly the same nature; it is not quite so black, from the greater admixture of debris of sandstone and clayslate.

In many parts of the plain of Nandiala, diamonds were formerly sought for, but the mines have for a long time ceased to be productive.

The failure of the mines of the Dekhin may perhaps be principally attributed to the cheapness and plenty of Brazil diamonds. Otherwise, from the vast extent of the rock in which they are found in India, there are scarcely any limits to the search for them. It may be assumed then:

1st. That the matrix of the diamonds produced in southern India, is the sandstone breccia of the "clay slate formation."

2d. That those found in alluvial soil are produced from the debris of the above rock, and have been brought thither by some torrent or deluge, which could alone have transported such large masses and pebbles from the parent rock, and that no modern or traditional inundation has reached to such an extent.

3d. That the diamonds found at present in the beds of the rivers are washed down by the annual rains.

It will be an interesting point to ascertain if the diamonds of Hindustan can be traced to a similar rock. It may also be in the power of others more favourably situated than the writer, to ascertain, if there be any foundation for the vulgar opinion of the continual growth of the diamond. Dr. Brewster's opinion is rather in favour of it than otherwise. It is certain that in these hot climates crystallization goes on with wonderful rapidity, and I hope at some future period to produce undeniable proofs of the re-crystallization of amethyst, zeolite and felspar, in alluvial soil.

LVIII. *Notices respecting New Books.*

THE Third Part of the Philosophical Transactions for 1826 has just appeared; the following are its contents:

On the coagulation by heat of the fluid blood in an aneurismal tumour. By Sir Everard Home, Bart. V.P.R.S.—On the mathematical theory of suspension bridges, with tables for facilitating their construction. By Davies Gilbert, Esq. V.P.R.S., &c.—On magnetic influence in the solar rays. By Samuel Hunter Christie, Esq. M.A. F.R.S.—On the mutual action of sulphuric acid and alcohol, with observations on the composition and properties of the resulting compound. By Mr. Henry Hennell.—On a method of expressing by signs the action of machinery. By Charles Babbage, Esq. F.R.S. —On the parallax of the fixed stars. By J. F. W. Herschel, Esq. M.A. Sec. R.S.—A formula for expressing the decrement of human life. By Thomas Young, M.D. For. Sec. R.S.—Account of an experiment on the elasticity of ice. By Benjamin Bevan, Esq.—Results of the application of Captain Kater's floating collimator to the astronomical circle at the observatory of Trinity College, Dublin, and remarks relative to those results. By the Rev. J. Brinkley, D.D. F.R.S. P.R.I.A.—On the means of facilitating the observation of distant stations in geodætical operations. By Lieutenant Thomas Drummond. —On the production and formation of pearls. By Sir Everard Home, Bart. V.P.R.S.—On burrowing and boring marine animals. By Edward Osler, Esq.—An account of some experiments relative to the passage of radiant heat through glass screens. By the Rev. Baden Powell, M.A. F.R.S.—The Bakerian Lecture: On the relations of electrical and chemical changes. By Sir Humphry Davy, Bart. Pres. R.S.—On the discordances between the sun's observed and computed right ascensions, as determined at the Blackman-street Observatory in 1821 and 1822; with experiments to show that they did not originate in instrumental derangement. Also, A description of the seven-feet transit with which the observations were procured, and upon which the experiments were made. By James South, Esq. F.R.S.—On the existence of a limit to vaporization. By M. Farady, F.R.S.—On electrical and magnetic rotations. By Charles Babbage, Esq. F.R.S. &c.— Case of a lady born blind, who received sight at an advanced age by the formation of an artificial pupil. By James Wardrop, Esq. F.R.S. Ed.—On the progressive compression of water by high degrees of force, with some trials of its effects on other fluids. By J. Perkins.—On the figure of the earth. By George Biddell Airy. M.A.

The Second Part of the Second Volume of the Memoirs of the Astronomical Society of London has just been published, the contents of which are as follows: viz.

On the latitude of the royal observatory at Greenwich. By John Pond, Esq.—On the determination of latitudes by observations of azimuths and altitudes alone. By M. Littrow.—Mémoire sur différens points relatifs à la théorie des perturbations des Planètes exposée dans la *Mécanique Céleste.* Par M. Plana.—Mr. John Ramage's description of his large reflecting telescopes.—On parallaxes. By J. J. Littrow.—On the co-latitude of the observatory of Stephen Groombridge, Esq., at Blackheath; determined by his own observations of circumpolar stars, reduced by the constant of refraction 58″, 133 at 45°.—Observations of the eclipses of Jupiter's satellites, made at Futty Ghur, on the Ganges, (N. Lat. 27° 21′ 35″) in 1824-5. By Major J. A. Hodgson.—A comparison of observations made on double-stars. By Professor Struve.—Observations of the occultation of Saturn on the 30th October, 1825. By R. Comfield, Esq. and J. Wallis, Esq.—Account of some observations made with a 20-feet reflecting telescope by J. F. W. Herschel, Esq., comprehending, 1. Description and approximate places of 321 new double and triple stars. 2. Observations of the second comet of 1825. 3. An account of the actual state of the great nebula in Orion, compared with those of former astronomers. 4. Observations of the nebula in the girdle of Andromeda.—Explanation of the method of observing with the two mural circles, as practised as present at the royal observatory at Greenwich. By John Pond, Esq.—Extracts from three letters, addressed by M. Gambart, Director of the royal observatory at Marseilles, to James South, Esq., M.A.S., respecting the discovery, and elements of the orbit, of a comet, which appears to be the same with that of 1772 and 1805.—Report of the Committee appointed by the Council of the Astronomical Society of London, for the purpose of examining the telescope constructed by Mr. Tulley, by order of the council.—Micrometrical Observations of the planet Saturn, made with Fraunhofer's large refractor, at Dorpat. By Professor Struve.—Summary of the observations made for the determination of the latitude of the observatory of Wilna. By J. Slawinsky.—Supplement to a former paper "On the latitude of the royal observatory at Greenwich." By John Pond, Esq.—Report of the Council of the Society to the sixth annual general Meeting.—An address delivered at a special general meeting of the Astronomical Society of London, on April 14, 1826, on presenting the gold medals to J. F. W. Herschel, Esq., J. South, Esq., and Professor

fessor Struve, by Francis Baily, Esq. President.—Appendix, containing the remainder of the tables for determining the apparent places of 2881 principal fixed stars, an account of which will be found at p. 339 of the present Number.

Just Published.

Geological and Historical Observations on the Eastern Vallies of Norfolk, by J. W. Robberds, jun. In one volume 8vo. price 4*s*. boards. Printed for Longman, Rees, Orme, Brown, and Green, London; and Bacon and Kinnebrook, Norwich.

Preparing for Publication.

Mr. Faraday has in the press an 8vo volume, to be entitled Chemical Manipulation; containing instruction to students in Chemistry relative to the methods of performing experiments, either of demonstration or research, with accuracy and success. It will be illustrated with numerous engravings of apparatus in wood.

Early in December: No. IX. of the Zoological Journal, containing a variety of papers in zoology, with an account of the life and writings and contributions to science, of the late Sir Stamford Raffles, F.R.S. &c. founder of the Zoological Society.

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

THE Royal Society re-assembled for the session of 1826–7, on the 16th of November,—the President, Sir H. Davy, in the chair.

The President announced that His Majesty had placed at the disposal of the Society, the suite of apartments in Somerset House lately occupied by the Commissioners of the Lottery, to be applied to any purposes to which the Society may wish to assign them. He also announced the resignation of W. T. Brande, Esq. one of the Secretaries to the Society.

Lieut.-Col. D. Denham, Capt. W. H. Smyth, R.N., and Nicholas Brown, Esq. were respectively admitted Fellows of the Society.

The Croonian Lecture, by Sir E. Home, Bart. V.P.R.S. was read, On the generation of the common oyster and the river muscle; with microscopical illustrations, by Mr. Bauer.

The reading was commenced of a paper, On a percussion shell, to be fired from a common gun, by Lieut.-Col. Millar; communicated by R. I. Murchison, Esq. F.R.S.

Nov.

Nov. 23. Charles Bell, Esq. was admitted a Fellow of the Society;—MM. Bouvard, Chevreul, and Dulong, were respectively elected Foreign Members; and the reading of Col. Millar's paper was concluded.

LINNÆAN SOCIETY.

Nov. 7th. A. B. Lambert, Esq. V.P. in the Chair.—A Continuation of Dr. Hamilton's Commentary on the *Hortus Malabaricus* was read. Jos. Woods, Esq. was elected a Member of the Council in the stead of Sir Stamford Raffles, deceased.

Nov. 21.—Part of a paper was read, intitled Remarks on the comparative anatomy of certain birds of Cuba, with a view to their respective places in the System of Nature, or to their relations with other animals. By W. S. MacLeay, F.L.S. &c.

In the introductory part of this paper the author (who is at present resident in the island of Cuba) insists on the great importance of studying comparative anatomy and natural arrangement in connection with each other, so that in the examination of particular organs the place held in nature by the animal to which they belong may be also investigated. The principles of arrangement laid down in Aristotle's *Historia Animalium* are then examined with reference to the principle, introduced by Mr. MacLeay, of studying the variation of structure in different animals in preference to classing them according to an arbitrary division of organs.

GEOLOGICAL SOCIETY.

A paper was read entitled, "Additional remarks on the nature and character of the limestone and slate composing principally the rocks and hills round Plymouth," by the Rev. Richard Hennah, F.G.S.

The author refers to his former paper on this subject, in which he confined his field of observation to the narrow tract between the Plym and the Tamar;—he now extends its limits to Mount Batten and Statten Heights, in a southerly direction. In this tract which forms the east side of Plymouth Sound, as well as the western side from Mount Edgcombe to Pudding Point, *animal remains* are imbedded in the slate. On the eastern side the superior beds are occasionally of an ochreous clay-slate containing thin veins of iron with trochites and stems of encrinites: these are associated with some peculiar fossil remains, which the author can assign to no class; they resemble the head of some plant or animal.

The lower beds consist of compact white or light gray slate inclosing the same animal remains which are found in the limestone

limestone and slate. An iron-stone bed occurs here which is used for pavements, and fragments of the same animal relics are discoverable in it: a great fissure in the cliff developes fossils of a new character, the nature of which does not appear to be determined.

From the above facts the author infers, that the slate which is prolonged beyond the Plymouth limestone, even as far *southward* as Whitesand Bay, is *not primitive:* but he remarks, that he has never perceived animal remains in the slate north of that limestone.

Extracts were read from letters from Captain Franklin, R.N. and Dr. Richardson, to Dr. Fitton, V.P.G.S. dated 5th of November 1825, at Fort Franklin, on the Great Bear Lake, N. America. Lat. 65° 12′ N.; long. 123° 5′ W.

Capt. Franklin states, that the expedition under his command had been so much favoured by the season of 1825, as to have accomplished some objects which he scarcely hoped to have attained within that time. Of these the most important were his having reached the sea in latitude 69° 29′, and longitude 135° 40′; and being enabled to see the direction of the coast, both east and west from the mouth of the Mackenzie River:—and while he was thus engaged on the Mackenzie, Dr. Richardson went round the northern shore of the Great Bear Lake, for the purpose of becoming acquainted with that part of it to which his course is to be directed in returning from the mouth of the Copper Mine River.—Capt. Franklin gives a general account of the structure of the tract on the course of the Mackenzie, through which he had passed; and Dr. Richardson describes the principal physical and geological features of the country traversed by the expedition,—the total distance being about 5100 miles.—The party, at the date of the letters, were established in their winter quarters.

MEDICO-BOTANICAL SOCIETY OF LONDON.

The first general meeting of this Society was holden on Friday the 13th of October at 8 o'clock P.M.,—Sir James MacGregor, K.T.S. Director-General of the Army Medical Board, President, in the chair.

The Director, Mr. Frost, delivered his oration, in which he congratulated the Society on the rapid advance it had made during the last session, and the great benefit it had derived from the unwearied zeal which many of its members exerted in its behalf. He also informed the meeting that their distinguished President had lately ordered "that no person shall be admitted to an examination to qualify him to practise in the Medical Department of the Army without having attended

amongst

amongst other branches of science lectures on Botany for six months; the salutary effects of which regulation would in a few years demonstrate its utility."

Sir James rose to address the meeting, and assured them that he was but performing his duty in enforcing the regulation just mentioned, or any other of a similar kind which might in any degree be conducive to the extension of practical and useful knowledge in that department, with the direction of which he had been intrusted; and concluded by moving that the thanks of the Society be given to the Director, and that he be requested to make his oration public.

A communication from H. B. M.'s Vice-Consul for Guatemala, Mr. Schenley, was read; and the meeting adjourned to Friday the 10th day of November 1826.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Thirteenth Annual Report of the Council.

In performing this annual duty, the council have again the pleasure of announcing the continued prosperity of this institution. Its progress is indeed slow and unattended by any brilliant transactions, but it is silently and unostentatiously advancing in the acquisition of a Geological knowledge of our county. More considerable and valuable additions to our cabinet of native minerals might from time to time be made, and much useful information might be obtained by minutely exploring various interesting localities; but the limited state of the Society's funds opposes an insuperable obstacle to the speedy completion of these important undertakings. If therefore this unscientific, but real difficulty, be taken into consideration, the wonder will then be, not how little, but how much has been accomplished.

Another year has again elapsed, but the school of mines is not established: and there appears at present little probability of the then proposed plan being carried into execution. The council regret its failure, since the hope of a speedy removal of a national reproach has been thereby postponed; but they will not yet despair of the eventual accomplishment of a measure, first suggested by this institution, and which it has never ceased to recommend. This Society, enrolling amongst its members a great portion of the rank and wealth of the county, should be foremost in promoting an institution of the first importance, both in an œconomical and scientific point of view; and for the establishment of which "*One and All*" should unite, who are interested in the honour and prosperity of Cornwall.

The museum, which in the foremost place commands the council's

council's attention, has been enlarged by the addition of another cabinet extending the whole length of one of the galleries: this has afforded sufficient space for the exhibition of several series of foreign specimens, which have been arranged by the curator with his usual neatness and judgement. The attainment of this object will afford but little interest to the scientific stranger; but to our native students it is of great importance, as displaying examples of the various and dissimilar mineral productions of our globe. To our constant and liberal correspondent, Wm. Maclure, Esq. of Philadelphia, we are indebted for another donation of minerals from the United States of America; consisting of various modifications of serpentine, of augite, maclurite, franklinite, red oxide of zinc, and other interesting specimens. Doctor Jer. Van Rensselaer, of New York, has presented to the Society several earthy and metallic minerals, which are peculiarly specified in the report of donations.

Capt. Wallis, of Bodmin, has also presented a series of specimens illustrative of the geology of the country between Hydrabad and Madras, accompanied by a descriptive account of the same, together with a map of this part of the peninsula of India.

To the kind contributions of several members, in compliance with the request made at the last annual meeting, the geological department has been recently indebted for many specimens of granite: but the varieties of this interesting formation are so numerous, that many more would be equally acceptable.

The department of simple minerals has also been enriched by the purchase in Cumberland of a series of splendid fossils, consisting of variously crystallized galena, blende, fluor spar, sulphate of barytes, arragonite, and other minerals.

Before concluding, the council take this opportunity of soliciting further donations from those members who have private collections: many may have refrained from presenting their spare duplicates, on the ground that our museum already possesses better specimens of the same kind: these however would be very acceptable, and would enable the Society to comply with the repeated applications that have been made by institutions, both at home and abroad, for an exchange of minerals.

Lastly, The council propose for the consideration of the general meeting the propriety of expending a portion of their income in the collection of geological specimens; thereby promoting the objects of the Society by enlarging the knowledge of our rock formations, and collecting several pieces of

the

the same rocks for the purpose of exchanges. Simple crystalline minerals cannot be obtained to any great extent, but at prices very far exceeding our funds, and indeed such Cornish minerals may be found in the collections of all extensive mineral dealers. Rock specimens, however, are not to be met with in such places, but they can be collected with comparatively little expense; and a series of granite, slate, and serpentine rocks would be the most valuable return the Society could make in their exchanges with scientific institutions. Moreover such intercourse cannot fail of being highly advantageous to both parties, and may be instrumental in the advancement of the science of geology.

(By order) H. S. Boase, *Secretary.*

Oct. 13, 1826.

The following Papers have been read since the last report:—On the granite of the west of Cornwall. By Joseph Carne, Esq. F.R.S.—On the sand banks of the northern shores of Mount's Bay. By H. S. Boase, M.D.—Some account of certain ancient circles and barrows on the summit of Botrea Hill, in the parish of Sancreed. By T. F. Barham, M.D.—On the changes which appear to have taken place in the primitive form of the Cornish peninsula. By John Hawkins, Esq., F.R.S.—On the temperature of mines. By Henry Boase, Esq.—An account of some circumstances connected with the heave of a copper lode by a flucan vein in the Consolidated mines, in the parish of Gwenap. By Mr. W. J. Henwood.—On the geology of the coast from Sennen Cove to the Land's End. By Joseph Carne, Esq. F.R.S.—On a singular exudation of gas in the Union mines, in the parish of Gwenap. By Mr. W. J. Henwood.—Observations on the suspension of the stannary courts. By Henry Boase, Esq.—On the importance of a deep adit from the northern coast. By R. Edmonds, Esq.—A notice relative to a new fusee for the blasting of rocks. By R. Collins, Esq.—An account of the quantity of tin produced in Cornwall in the year ending with the Midsummer quarter, 1826. By Joseph Carne, Esq., F.R.S.—An account of the produce of the copper mines of Cornwall, in ore, copper and money, in the year ending the 30th June, 1826. By Mr. Alfred Jenkyns.

Donations of Minerals, since the last Anniversary.

Various modifications of serpentine, augite, maclurite, franklinite, red oxide of zinc, &c. from the United States of America. By Wm. Maclure, Esq. of Philadelphia.—Colophonite, garnets, beryl in the matrix, coccolite, sulphate of barytes, red oxide of zinc, hornblende, rose-quartz, &c. By Jer. Van

Van Rensselaer, Esq. M.D. of New York.—Specimens illustrative of the geology of the country between Hydrabad and Madras, with a map. By Capt. Wallis.—Specimens of wavellite. By the right honourable lord De Dunstanville.—Grawacké from near Launceston. By Davies Gilbert, Esq. M.P. —Specimens of the sand banks of Mount's Bay and their substrata, with a section. By H. S. Boase, M.D.—Several varieties of granite from the Land's End district. By Joseph Carne, Esq.—Specimens of granite, illustrative of his communication on this subject. By T. F. Barham, M.D.—Varieties of granite and slate from the neighbourhood of Tregonning Hill. By George S. Borlase, Esq.—A specimen of beryl from St. Michael's Mount. By Mr. Edward A. Crouch.—Chalcedony investing quartz crystals, from Pednandrea mine: crystallized quartz from Rodney mine: earthy red copper ore from Wheal Caroline: and a gryphite in limestone from near Plymouth. By H. S. Boase, M.D.—Three specimens of celestine from Girgenti in Sicily. By Joseph Carne, Esq.—Granite from Scilly. By John Armstrong, jun., Esq.—Plumbago from Borrowdale. By R. Edmonds, Esq.—Yellow fluor from Cumberland; swimming quartz and uranite from North Downs. By Wm. M. Tweedy, Esq.

A metallic pan and cover, about 15 inches in diameter, found at the depth of 12 feet in an old stream work near St. Columb. The metal is very good tin, nearly equal to grain. This vessel was probably used for culinary purposes, and at a period previous to the introduction into this country of the alloys of tin with copper and with lead. By H. S. Boase, M.D.

At the Anniversary Meeting, held on the 13th of October, 1826; Davies Gilbert, Esq., M.P., V.P.R.S., President, in the chair;—the report of the council being read,

It was Resolved,—That it be printed and circulated amongst the members. That the thanks of the Society be presented: 1. To the authors of the various papers; 2. To the donors of minerals, books, &c.; 3. To the officers of the Society; Lastly, That another volume of Transactions be forthwith published, and that the new council be requested to take immediate measures for that purpose.

The following gentlemen were then elected officers and council for the present year. *President:* Davies Gilbert, Esq. M.P., &c.—*Vice-Presidents:* Sir John St. Aubyn, Bart.; Sir Charles Lemon, Bart.; William T. Pread, Esq.; Rev. Uriah Tonkin.—*Secretary:* Henry S. Boase, M.D.—*Treasurer:* Henry Boase, Esq.—*Librarian:* T. F. Barham, M.D.—*Curator:* Edward C. Giddy, Esq.—*Assistant Secretary:* Ri-

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chard Moyle, Esq.—*Council:* George S. Borlase, Esq.; Joseph Carne, Esq.; Stephen Davey, Esq.; Richard Edmonds, Esq.; Robert W. Fox, Esq.; George Grenfell, Esq.; Humphry Grylls, Esq.; George D. John, Esq.; William M. Tweedy, Esq.; Michael Williams, Esq.

LX. *Intelligence and Miscellaneous Articles.*

INDIAN GRAPHITE.

THE following account of the graphite of the Himalaya was lately read before the Asiatic Society of Bengal.

Dr. Abel observes, that the plumbago of mineralogists is divided into two species, the scaly and compact; their names depending on the relative size of the grains disclosed on their fracture, or, according to Mohs, "the former comprehending those which are still discernible, while in the latter they are withdrawn from observation." According to this definition, the Himalayan mineral belongs to the order of compact graphites.

The Himalayan graphite is found on the surface of a hill composed of highly carburetted mica slate, a locality which corresponds with that of Aberdeenshire and other countries, according to Mohs, Jameson, and other mineralogists.

Graphite varies so much in specific gravity, that it is impossible to take its weight as a perfect criterion of its purity: the lowest specific gravity of the specimens examined by Dr. Abel was 2·268, and the highest 2·488.

The following list is given, by Dr. A. of the specific gravity of the graphite, by various authors, as well as from various countries:—

Kirwan	1·987	to	2·267
Brisson	2·150	..	2·456
Henry	2·089	..	2·246
Ure and Jameson	1·9	..	2·4
Thomson	1·987	..	2·267
Thenard	2·08	..	2·26
Borrowdale graphite, in Dr. A.'s possession			2·267
Spanish ditto ditto			1·379
Ava ditto ditto			2·246
Ceylon ditto ditto			2·000
Himalayan ditto, No. 1,			2·268
——— 2,			2·375
——— 3,			2·463
——— 4,			2·488

Although

Although the specific gravity is not of itself a sufficient criterion, it is found to bear a general relation to the purity of the mineral; and the Borrowdale and Spanish varieties, which contain the largest proportion of carbon, have the lowest specific gravity. In the same manner, of the Himalayan specimens, Nos. 1 and 2, which are the lightest, present the finest grain, and are freest from earthy admixture; and the specimens from Ava and Ceylon approximate to Borrowdale graphite, both in weight and external character. None of the specimens of the Himalayan graphite have a metallic lustre unless scraped, and it is then of inferior brilliancy to the other varieties. The smallest nodules have the finest grain, and make the blackest streak: by boiling in oil, the streak is deepened in colour and rendered softer. These nodules are hollow in the centre; the larger pieces have a slaty fracture, and exhibit a considerable proportion of siliceous admixture. The smaller specimens, deflagrated with nitre, afforded indications of earthy matter, as did the larger in more considerable proportion. The best specimens left a residuum of oxide of iron of 52 per cent; and about a similar proportion, or 5 per cent, was left after roasting for five hours. There had not been time for a regular analysis, but Dr. A. considers the following as the mineralogical characters of the graphite of the Himalaya, taking the smaller specimens as a standard. Its colour is charcoal black; it occurs in rounded and angular fragments; internally it is barely glistening, externally dull and earthy; its fracture is very fine grained. The fragments are angular; its streak is shining and metallic; it is imperfectly sectile; it is frangible; it writes and soils; it feels rather greasy. The specific gravity is 2·268 to 2·375.—*Asiatic Journal.*

ON THE COMPOSITION OF THE MOSAIC GOLD, OR OR-MOLU, DISCOVERED BY MESSRS. PARKER AND HAMILTON.

The resemblance of this alloy to pure gold has attached to the discovery of it an importance of no ordinary kind. Although it is an alloy of zinc and copper, yet great care and experience are necessary to its production. The following is the exact method given by the patentees.

Take equal quantities of zinc and copper, and melt them at the lowest temperature at which copper will fuse. Having mixed them perfectly by stirring, add zinc in small portions till the alloy in the crucible assumes a yellow colour like brass, then continue adding the zinc till the colour changes to a purple or violet, and becomes perfectly white, which is the colour necessary to its perfection. It may then be cast into

 ingots,

ingots, or into any required form, and when cold, it will have the appearance of an alloy of fine gold and copper.

The great art in making the alloy consists in working with the lowest temperature; for if the temperature is too great the zinc will fly off in fumes, and the product will be spelter or hard solder. From this cause it is difficult to make the alloy preserve its character when remelted. The alloy consists of a hundred parts of copper, and of from fifty-two to fifty-five parts of zinc.—*Edin. Journ. of Science.*

ACCOUNT OF A PATENT SUBSTITUTE FOR LEATHER. INVENTED BY MR. THOMAS HANCOCK.

In a former patent, Mr. Hancock proposed to form a substitute for leather, by depositing caoutchouc in a fluid state upon loose fibres of wool, cotton, or flax, felted or matted together. In the present patent, he uses a *woven cloth*, made of wool, cotton, or flax. When this cloth is stretched upon a flat surface, the composition to be presently described is spread over it. Above the composition, a uniform layer of wadding, made of cotton, flax, wool, silk, or hair, is to be laid, and the whole pressed between a pair of rollers, in order to force the fluid composition among the fibres. It is then to be dried at a temperature not exceeding 80° or 90° of Fahrenheit.

Mr. Hancock has given us the two following compositions, to be used according to circumstances.

First composition.—Dissolve two pounds of caoutchouc in one gallon of oil of turpentine and highly rectified coal tar. Add six ounces of black resin, two pounds of strong glue size, and one pound of yellow ochre, whitening, or powdered pumice.

Second composition.—Dissolve 1½lb of caoutchouc as before, and having melted and mixed one pound of glue size and resin in a steam bath, add the dissolved caoutchouc to it, stirring while mixing them. The whole must then be strained through a sieve.

The *first* of the above compositions must be used when a cheap and stiff substance is required, and the proportions may be one-third part whitening or glue; but when a strong and pliant substance is wanted, the *second* composition, in which the caoutchouc predominates, is to be preferred.

A substance like leather may be formed by joining together several thicknesses before they are dry. When leather for the soles of shoes is required, Mr. Hancock proposes to use, as the groundwork, wool and cotton in equal quantities. For pipes, straps, &c. he proposes chopped hemp and cotton or flax;

flax; and when smooth surfaces are wanted, the substance must be pressed between polished metallic plates.—*Edin. Journ. of Science.*

LUMINOUS STONES.

At a late meeting of the Philomathic Society of Paris, M. Becquerel produced a stone possessed of very singular properties. It was a species of chalk, and was sent by M. Leman from Siberia, where it was met with in some granite rocks.

This stone when placed in the dark exhibits a very remarkable phosphoric light, which increases in proportion as the temperature is raised. Its lustre, *cæteris paribus*, becomes greater if it is immersed in water. M. Becquerel, having put it into boiling water, found that it became so bright that he could distinguish printed characters close to the transparent vessel which contained it. In boiling oil the effect was still further augmented, and in boiling mercury it cast a light so brilliant that he could read at the distance of five inches. M. Becquerel was desirous of increasing the temperature in order to ascertain the effects, but was fearful of destroying the stone.

M. Eyrès remarked upon this occasion a curious fact. Sir John Mandeville, the author of Travels performed in the middle of the fifteenth century in Central Asia, relates that he found at the entrance of a city in Great Tartary two columns surmounted with stones, which shed a brilliant lustre in the dark. His statement has hitherto been classed amongst fables; but the above-mentioned fact affords, M. Eyrès observes, some ground for believing that he may not have told an untruth.

SYSTEMA ORBIS VEGETABILIS.

A work under this title has recently been commenced by the learned and ingenious Swedish philosopher Fries, in which he proposes to arrange the whole vegetable kingdom, according to the views entertained by him, Dr. Nees ab Esenbeck, and some other naturalists *. Our readers will recollect that the doctrines of affinity and analogy are very carefully studied and distinguished by the promoters of those views; and it is certain that they have already contributed not only towards a more philosophical arrangement of natural bodies, but one also more tangible in practical investigation. M. Fries is well known by his laborious work on the *Fungi*: a tribe of vegetables, indeed, holding a low scale in creation, but capable of illustrating the advantages of the system pursued by the author. "M. Fries," observes Mr. W. S. Mac-

* See p. 81 of the present volume.

Leay,

Leay, "has been able to give so connected and symmetrical an outline of what he considers to be the natural distribution of *Fungi*, as, at least in my opinion, to merit the careful attention of zoologists as well as botanists." In the 14th volume of the Linnæan Transactions, Mr. MacLeay has successfully proved the same laws to be applicable to the natural distribution of insects; and more recently, in the same Transactions, Mr. Vigors has extended them, in an able manner, to the orders and families of birds.

In the present work, M. Fries confines himself to the genera; which he intersperses with numerous and valuable observations. The first part, recently published, contains 448 genera of *Fungi*, Lichens, *Byssaceæ*, and *Algæ*; four great groups, which he arranges in two classes, FUNGI, and ALGÆ. The Lichens he considers as aërial *Algæ*.—*Edin. Journ. of Science.*

BITBURG METEORIC IRON.

This iron, described in our lxvth volume, page 401, has lately been re-examined by Dr. John, with the following results.

Essential constituents:

Iron	78,82
Sulphur	4,50
Nickel	8,10
Cobalt	3,00
Selenium	traces.

Accidental constituents derived from the smelting.

Silicium	0,08	combined with the iron.
Carbonaceous matter	traces	
Silica, alumina, and oxide of iron	5,50	
	100,00	

LIST OF NEW PATENTS.

To Benjamin Newmarch, of Cheltenham, esquire, for his improvements on fire-arms.—Dated the 7th of November 1826.—6 months allowed to enrol specification.

To Edward Thomason, of Birmingham, goldsmith and silversmith, for improvements in the construction of medals and coins.—9th of November.—2 months.

To Henry Charles Lacy, of Manchester, coach-master, for a new apparatus on which to suspend carriage bodies.—18th of November.—6 months.

To Bennett Woodcroft, of Manchester, silk manufacturer, for improvements in wheels and paddles for propelling boats and vessels.—18th of November.—6 months.

Results

Results of a Meteorological Journal for October 1826, *kept at the Observatory of the Royal Academy, Gosport, Hants.*

General Observations.—With the exception of two or three days, the weather this month has been generally calm and mild for the season: the state of the atmosphere, however, has been often humid and suddenly condensed by crossing currents of wind, which induced frequent, but not heavy showers of rain.

Between the *maximum* temperature of the air of the 6th instant, which was the coldest day, and that of the 11th, there was a difference of 16 degrees: and the difference in the *minimum* temperature of the nights of the 5th and 19th was 22 degrees: the former night was very cold to the feelings at this season, and the latter sultry.

The 22nd was a stormy day, and damage was done in several northern counties by the discharges of electric fluid: the lightning, which was also seen in this neighbourhood and at Christchurch early in the morning, was generated by the inosculation of two winds of unequal temperatures, crossing at right angles from N.E. and S.E. Much lightning emanated from the passing clouds on this and the subsequent evening.

At 9 A.M. on the 24th two *parhelia* and a solar halo presented themselves in the South-east quarter; and two rainbows appeared in the afternoon, one of which, independently of the exterior bow that accompanied it, showed *seven* bows of colours, two of them were formed by a strong reflection of the colours, being similar, but not quite so vivid: they rose in semicircular arcs in the following order; viz. yellow, crimson, yellow, crimson, blue, green, red.

The mean temperature of the external air this month is $3\frac{1}{3}$ degrees higher than the mean of October for the last ten years; this, therefore, is the warmest October since 1818. Spring water arrived at its warmest state on the 12th; since that time its temperature has decreased 3-5ths of a degree; and as the sun has now advanced more than half way in South declination, its temperature will decrease in proportion to that of the ground.

The atmospheric and meteoric *phænomena* that have come within our observations this month, are two *parhelia*, one solar and three lunar halos, fourteen meteors, four rainbows, lightning on three different days, and thunder on one; and two gales of wind; namely, one from the East, the other from South-west.

Numerical

Numerical Results for the Month.

		Inches.	
Barometer	Maximum	30·17,	Oct. 14th—Wind N.E.
	Minimum	29·42,	Ditto 25th—Wind W.

Range of the mercury . . 0·75.

	Inches.
Mean barometrical pressure for the month	29·904
——— for the lunar period ending the 1st inst. .	29·897
——— for 14 days, with the Moon in North declin.	29·919
——— for 15 days, with the Moon in South declin.	29·875
——— for the lunar period ending the 30th inst. .	29·902
——— for 15 days with the Moon in North declin.	29·836
——— for 14 days with the Moon in South declin.	29·969
Spaces described by the rising and falling of the mercury	4·650
Greatest variation in 24 hours	0·560
Number of changes	28·

Thermometer	Maximum	68°	Oct. 11th, 13th, & 21st.
	Minimum	38	Do. 5th—Wind N.W.

Range	30
Mean temp. of the external air	56·26
——— for 31 days with the Sun in Libra	59·19
Greatest variation in 24 hours	21·00
Mean temp. of spring water at 8 o'clock A.M. . . .	55·34

De Luc's *Whalebone Hygrometer.*

	Degrees.
Greatest humidity of the air .	100 in evenings of 22d & 23d.
Greatest dryness of ditto . . .	44 in the aftern. of the 6th.
Range of the index	56
Mean at 2 o'clock P.M. . . .	64·8
—— at 8 o'clock A.M. . . .	74·1
—— at 8 o'clock P.M. . . .	77·4
—— of three observations each day at 8, 2, and 8 o'clock	72·1

Evaporation for the month	1·60 inch.
Rain in the pluviameter near the ground .	2·225
Rain in ditto 23 feet high	2·060

Prevailing wind, N.W.

Summary of the Weather.

A clear sky, 3; fine, with various modifications of clouds, 13; an overcast sky without rain, 10; rain, 5.—Total 31 days.

Clouds.

Cirrus.	Cirrocumulus.	Cirrostratus.	Stratus.	Cumulus.	Cumulostr.	Nimbus.
16	9	27	2	20	27	16

Scale of the prevailing Winds.

N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Days.
1	1	$4\frac{1}{2}$	4	1	7	5	$7\frac{1}{2}$	31

A METEORO-

Gosport, at half-past Eight o'Clock, A.M.						Clouds.									Height of Barometer, in Inches, &c.		Thermometer				Rain.		Weather.		
																	London.								
Days of Month, 1826.	Barom. in Inches, &c.	Thermo.	Temp. of Sp. Water.	Hygrom.	Wind.	Cirrus.	Cirrocum.	Cirrostr.	Stratus.	Cumulus.	Cumulost.	Nimbus.	Evaporation.	Rain near the ground.	Lond. 1 P.M.	Bost. $8\frac{1}{2}$ A.M.	8 A.M.	Noon.	11 P.M.	Boston. $8\frac{1}{2}$ A.M.	London.	Boston.	London.	Boston.	Wind.
Oct. ● 1	29·80	58	55·50	75	W.	...	1	1	...	1	1	1	...	0·300	29·80	29·37	57	64	49	54·5	...	...	Cld^y, rain	Misty	calm
2	29·90	55	......	69	E.	1	...	1	1	1	1	...	...	......	29·95	29·50	48	61	54	49	...	...	Fair	Fine	W.
3	29·90	54	......	69	NW.	...	...	1	...	1	...	...	0·10	......	29·91	29·45	48	59	50	53·5	...	...	Cloudy	Cloudy	W.
4	29·78	54	......	66	W.	1	1	1	...	1	1	1	...	·235	29·77	29·23	50	55	45	53	...	...	Cloudy	Fine	W.
5	29·76	47	......	62	NW.	1	...	1	...	1	1	...	...	......	29·88	29·35	43	54	44	49·5	...	...	Fair	Fine	NW.
6	30·07	45	55·35	65	NW.	1	...	1	...	...	...	...	·20	......	30·10	29·73	38	52	46	42	...	...	Fair	Fine	NW.
7	30·09	55	......	70	S.	...	...	1	...	1	1	1	...	·035	30·08	29·65	48	58	56	50	...	...	Cloudy	Fine, rain *p.m.*	NW.
☽ 8	29·93	58	......	78	SW.	...	...	1	...	1	1	1	...	·200	29·88	29·50	58	60	55	55	...	·01	Rain	Fine, rain at night	calm
9	29·81	55	......	63	W.	1	1	1	...	1	1	1	·15	·140	29·79	29·35	50	56	46	48	...	·09	Cld^y r^n n^t	Rain	W.
10	29·72	58	......	91	SW.	1	...	1	...	1	1	1	...	·050	29·69	29·20	51	64	59	52	...	·10	Rain	Rain	W.
11	30·08	62	......	74	W.	1	...	...	...	1	1	1	...	·010	30·07	29·54	58	64	60	57·5	...	·03	Cloudy	Fine	W.
12	30·13	62	55·60	76	SW.	1	...	1	...	...	1	1	·10	·010	30·10	29·57	60	63	60	58	...	...	Cloudy	Cloudy	W.
13	30·06	63	......	76	SW.	1	...	1	...	...	1	...	...	......	30·16	29·40	60	60	50	62	...	...	Cloudy	Fine	W.
14	30·17	55	......	70	NE.	...	...	1	...	...	1	...	...	......	30·18	29·80	48	58	54	47	...	...	Cloudy	Fine	W.
○ 15	29·92	60	......	73	SE.	1	1	1	...	1	1	...	·25	......	29·86	29·55	57	63	55	57	...	...	Cloudy	Cloudy	S.
16	29·60	60	......	74	SE.	...	...	1	...	...	1	1	...	·020	29·64	29·15	58	64	48	57	...	...	Do.sh^{s}th^r	Fine	S.
17	29·94	58	......	55	SE.	...	...	...	...	1	1	...	...	......	30·00	29·60	45	58	52	46	...	...	Fair	Fine	W.
18	29·92	58	55·50	71	NE.	...	...	1	...	1	1	1	·20	·030	29·98	29·62	54	60	56	54·5	...	...	Cloudy	Fine	SE.
19	29·90	60	......	74	E.	...	...	1	...	...	1	...	...	......	30·00	29·66	58	60	58	57	...	...	Cloudy	Fine	E.
20	29·88	62	......	72	E.	1	...	1	...	...	1	1	...	......	30·00	29·55	58	60	59	57·5	...	...	Cloudy	Cloudy	E.
21	29·93	61	......	84	SE.	1	1	1	1	1	1	1	·10	·065	29·99	29·56	60	65	58	57·5	...	...	Fair	Cloudy	E.
22	29·90	61	......	85	E.	1	1	1	...	...	1	1	...	·360	29·94	29·45	59	64	58	59	...	...	R^n, light^g	Do. rain *a.m.* & *p.m.*	calm
23	29·86	61	......	88	SE.	1	1	1	...	1	1	...	...	·020	29·90	29·40	57	64	56	59	...	·28	Cld^y s^r th.	Cloudy, rain *p.m.*	calm
☾ 24	29·92	56	55·10	90	SW.	1	...	1	...	1	1	1	·15	·300	29·88	29·44	55	60	54	53	...	·25	Fair	Cloudy	W.
25	29·44	57	......	82	SW.	...	...	1	...	1	1	1	...	·250	29·42	29·82	53	59	45	56	...	·06	Rain	Rain	W.
26	29·43	48	......	66	NW.	1	1	1	...	1	1	...	...	......	29·46	29·02	44	51	41	46	...	·04	Fair	Fine	W.
27	29·62	51	......	86	SW.	...	...	1	...	...	1	1	·15	·200	29·60	29·20	44	51	46	45·5	...	...	Rain	Rain	W.
28	30·09	45	......	74	NW.	...	...	1	...	1	1	...	...	......	30·10	29·62	41	51	45	48	...	·24	Fair	Fine	W.
29	30·16	52	......	73	NW.	...	1	...	...	...	...	...	...	......	30·10	29·71	47	53	51	50	...	...	Cloudy	Fine, rain *p.m.*	W.
30	30·08	57	......	81	NW.	...	...	1	...	...	...	...	...	......	30·11	29·70	51	53	55	51	...	·09	Rain	Fine	W.
● 31	30·08	50	55·00	65	NW.	...	...	...	...	1	1	...	·20	......	30·10	29·72	47	51	44	45	...	...	Fair	Fine	NW.
Aver.:	29·899	56·06	55·34	74·1		16	9	27	2	20	27	16	1·60	2·225	29·92	29·49	52	58	52	52·5	2·50	1·19			

INDEX TO VOL. LXVIII.

THE END.

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1826.

NEW SERIES

OF

THE PHILOSOPHICAL MAGAZINE.

The Editor of the Philosophical Magazine has to announce to his friends and the public, that on the 1st of January next, he will commence, in conjunction with Mr. Richard Phillips, F.R.S. &c. a *New and Combined Series* of the Philosophical Magazine and Annals of Philosophy; in conformity with an arrangement entered into with the Proprietors of the *Annals*.

The circumstances which have rendered this junction expedient, will be found detailed in the Prospectus of the new Journal annexed to the present Number. The Editor of the Philosophical Magazine has gratefully to acknowledge the Contributions by which he has been enabled so materially to raise the character of the work since it has been conducted by him; and in announcing the future co-operation of a gentleman of acknowledged talent and reputation in science, he feels confident not only that he may rely upon a continuance of the same favours, but that the consequent improvement of the Journal will give it additional claims to support.

Shoe-Lane, London,
Nov. 29, 1826.

☞ Communications for The Philosophical Magazine and Annals of Philosophy, to be addressed to the Editors, care of Mr. R. Taylor, Shoe-Lane.